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The American Historical Review

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In This Issue

This issue begins with three essays on German and Italian fascism. In the first, **Thomas Childers** seeks in the language of everyday politics clues to the failure of liberal democracy in Weimar Germany. Such an approach offers a way out of the present impasse in the so-called *Sonderweg* debate over whether or not the rise of fascism can be attributed to the persistence in early twentieth-century German bourgeois political culture of "pre-industrial residues" that prevented the development of a modern liberal democratic tradition. Revisionist historians disagree with the assumptions about modernization that underlie orthodox analyses of the *Sonderweg*. A homogeneous *Mittelstand* never existed in Imperial Germany, they point out; the political vocabulary of *Stand*, or estate, was prescriptive. While Childers agrees that the idiom of the *Stand* did not reflect "objective" social conditions, he argues that language itself is a constituent part of social reality. As corporatist terminology increasingly dominated political life between 1919 and 1933, all German parties were forced onto a linguistic terrain most congenial to conservative and fascist politics.

In a complementary article on early twentieth-century Italy, **Walter L. Adamson** argues that Benito Mussolini took the essence of his cultural politics from the avant-garde "modernist" movement, particularly the *La Voce* group in Florence. Mussolini's vision of a "new Italy," an Italy that was to be deeply spiritual and the product of purifying, "regenerative" violence, owed much to the major participants in *La Voce* and its predecessor, *Leonardo*—Giovanni Papini, Giuseppe Prezzolini, and Ardengo Soffici. Yet, like modernists elsewhere, these men were unable to translate their visions into concrete politics. Not until the 1920s did Mussolini, a long-time friend of Prezzolini, succeed in politicizing modernist myth-making at the level of mass politics.

Within Italy, Renzo De Felice's multivolume biography of Mussolini has generated both scholarly and public debate. **Borden W. Painter, Jr.**, argues that De Felice's view of Mussolini and his rejection of "generic fascism" must be understood within the context of Italian intellectual and political life, especially the official antifascism that has been an important part of Italian society since the end of World War II. Painter focuses on the course of the controversy engendered by De Felice's ideas and concludes that, although De Felice remains controversial, his influence on the historiography of fascism in Italy and elsewhere must be taken into account by any student of the subject.

Patricia Ebrey attributes shifts in Chinese mortuary customs in the tenth through fifteenth centuries to a complex interaction of popular religion, the ideas of the highly articulate, and the efforts of the state to regulate behavior. The rise and decline of cremation in China has generally been explained as a result of the introduction of Buddhism into Chinese culture and the later successful reassertion of Confucianism. A closer analysis of the incidence of cremation suggests that the religious ideas that facilitated the acceptance of cremation came as much from folklore about ghosts and bones as from Buddhist doctrine; the circumstances that favored cremation were as much migration and urbanization as the spread of Buddhist-run crematories. The decline of cremation similarly owed as much to anti-foreign sentiments as to anti-Buddhist ones.

According to **Fred Matthews**, Allan Bloom's much-discussed best seller, *The Closing of the American Mind*, offers a powerful challenge to the assumptions of most contemporary professional historians. Indeed, Bloom's call for a return to timeless verities established by deductive reasoning from classic texts constitutes an assault on the value and sufficiency of historical consciousness itself. Matthews's response focuses on Bloom's indictment of the work of Charles A. Beard and Carl Becker, and of the pragmatic philosophy that has been the implicit metaphysics of most "liberal" historians. He attempts to "place" Bloom in recent intellectual history and also to deal with some of the issues of logic and evidence raised by his condemnation of modern scholarship. From an epistemological standpoint, Matthews argues, the deepest conflict is not between politically based "Right" and "Left" but between "absolutist" or "universalist" theories and those that assume all things change with time and context.

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Contributors

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Thomas Childers, professor of history at the University of Pennsylvania, is the author of *The Nazi Voter: The Social Foundations of Fascism in Germany 1919–1933* (1983) and editor of two other volumes on National Socialism—*The Formation of the Nazi Constituency* (1987) and, with Jane Caplan, *Reevaluating the Third Reich: New Controversies, New Interpretations* (1990). He received his doctorate from Harvard University in 1976 and is now studying language and politics in the Weimar Republic.

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press that concern Chinese family rituals: a translation of *Chu Hsi's Family Rituals* and *Ritual and Custom in Imperial China: A Social History of the Confucian Discourse on Family Rituals*.

Fred Matthews studied with Henry May at the University of California, Berkeley, and with Donald Fleming at Harvard University, and now teaches history at York University in Toronto. He has written *Quest for an American Sociology* (1977), *Northern California Railroads* (1984), and published a number of articles in intellectual history, including "Hobbesian Populism," in the *Journal of American History* (June 1985). The AHR essay grew out of his interest in the philosophical bases of contemporary historiography. In the course of responding to Allan Bloom's book, he has become involved in a larger examination of the state of contemporary knowledge about human nature and the philosophical grounding of the "absolutist" and "relativist" positions within contemporary academe.

Professor of history and chair of the department at Trinity College (Connecticut), **Borden W. Painter, Jr.**, received his doctorate from Yale University in 1965. He edited and wrote the introductory essay for a special issue of the *Cesare Barbieri Courier* (Trinity College, 1980) called "Mussolini and Italian Fascism," and organized a conference on the same subject in October 1982, which brought a number of scholars to Trinity, including Renzo De Felice. The papers from that conference appeared in the *Italian Quarterly* (Summer 1983). Professor Painter is currently doing research at the Archivio Centrale dello Stato in Rome on the three versions of the Exhibition of the Fascist Revolution (La Mostra della Rivoluzione Fascista): 1932–1934, 1937–1939, and 1942–1943.

The Social Language of Politics in Germany: The Sociology of Political Discourse in the Weimar Republic

THOMAS CHILDERS

FOR OVER A DECADE NOW, historians of modern German history have been caught up in what has come to be known as the *Sonderweg* debate. Although the controversy has many dimensions, at its core is a dispute about the extent to which the profoundly troubled political development of Germany in the twentieth century was produced by a partial, flawed, or even unsuccessful modernization. More specifically, it has turned on the question of whether the persistence of "pre-industrial residues," in either the form of "pre-modern traditions" or "pre-industrial groups or elites," fundamentally distorted German political culture after the middle of the nineteenth century, creating severe structural faults in German society and politics that would contribute directly to the failure of liberal democracy and ultimately lead to the rise of fascism.¹

Although the *Sonderweg* controversy has been fought primarily on the embattled terrain of the *Kaiserreich's* history, it has ranged both chronologically and thematically across the Imperial, Weimar, and National Socialist periods. In the process, it has broadened the horizons of research in the historiography of modern Germany and established the parameters of a theoretical debate whose implications reach far beyond the frontiers of German Central Europe. Recently, however, the debate has reached a methodological and theoretical impasse. In this essay, I would like to propose a possible way out, one that provides a fresh perspective on the issues involved and suggests new areas of research and debate.

THE SONDERWEG DEBATE, as students of German history are well aware, has been gathering momentum (and venom) since the mid-1970s. It started when a number of British historians launched a frontal assault on the theoretical assumptions and empirical foundations of what had become "the new orthodoxy"² in the historiography of modern Germany and the analysis of fascism in its German context. This new orthodoxy had emerged a decade earlier in the aftermath of the Fischer

I would like to thank Margaret Anderson and Jane Caplan for their helpful readings of this essay.

¹ The best guide to the *Sonderweg* debate remains Robert G. Moeller, "The *Kaiserreich* Recast? Continuity and Change in Modern German Historiography," *Journal of Social History*, 17 (1984): 442–50. See also James N. Retallack, "Social History with a Vengeance? Some Reactions to H.-U. Wehler's 'Das Deutsche Kaiserreich,'" *German Studies Review*, 7 (1984): 655–83; Roger Fletcher, "Recent Developments in West German Historiography: The Bielefeld School and Its Critics," *ibid.*, 451–80; Helga Grebing, *Der 'deutsche Sonderweg' in Europa 1806–1945: Eine Kritik* (Stuttgart, 1986).

² The terms "new orthodoxy" and "new revisionism" were first coined by James J. Sheehan in a review published in the *Journal of Modern History*, 48 (1976): 567.

controversy in the works of a new generation of German historians determined to overturn the traditional conservative historiography of the Imperial, Weimar, and National Socialist periods.³ All German history in the postwar era has been viewed, with quite mixed results, through the prism of the Nazi experience. In the 1960s, Hans-Ulrich Wehler, Heinrich August Winkler, Jürgen Kocka, and Hans-Jürgen Puhle decided to confront that experience directly, without the apologist reasoning of their predecessors. First, they turned away from the traditional emphasis on foreign policy. Borrowing heavily from American social science and Weberian theory, they focused on the evolution of German economic and social structures and on domestic political development. In the course of their research, they came to view National Socialism not as an inexplicable aberration from German traditions but as the continuation of a "flawed" socio-political development, the roots of which could be traced to the failure of political liberalism in 1848 and thereafter, as well as to the "lateness" of both German unification and industrialization. Central to this new orthodoxy is an emphasis on what Kocka describes as "the powerful persistence of pre-industrial, pre-capitalist traditions" in Germany. Indeed, adherents of this new orthodoxy view the lingering of such "pre-industrial, pre-capitalist" traditions as responsible for the tragic deviation from presumably "healthy," Western norms that culminated in the rise of National Socialism.⁴

The orthodox analysis of this German *Sonderweg* concerns two basic areas of inquiry. It focuses on the timing of German industrialization and the subsequent failure of the *Bürgertum* to gain political power. In contrast to Britain and France, German industrialization came, to use Ralf Dahrendorf's classic formulation, "late, fast, and thoroughly,"⁵ producing within two generations one of the world's most advanced industrial economies but leaving in its wake substantial residues of pre-industrial, indeed, pre-capitalist, modes of production and distribution. Artisans, small shopkeepers, and small family farmers—members of the traditional middle class (*Mittelstand*)—found themselves threatened by the rapid development of large-scale corporate capitalism on the one hand and the emergence of an increasingly well-organized and apparently militant socialist working-class movement on the other. Also under attack, the deeply entrenched elites in the army, bureaucracy, and East Elbian aristocracy sought to maintain their monopoly on political power through a series of manipulative strategies, including negative integration (the *Kulturkampf* and the anti-socialist laws of 1878–1890), *Sammlungspolitik* (the "marriage of rye and iron"), and social imperialism (*Weltpolitik*). Lacking a tradition of successful bourgeois revolution, the upper middle class (so the orthodox argument goes) was gradually coopted—socialized in pre-industrial values by the conservative educational system, the army, and bureaucracy—until it became the "feudalized bourgeoisie" so witheringly depicted in Heinrich Mann's *Der Untertan* (1918). The lower middle class, on the other hand, was effectively mobilized for conservative ends by anti-Marxist, anti-Semitic, corporatist slogans

³ On the controversy surrounding Fritz Fischer's *Griff nach der Weltmacht*, see J. A. Moses, *The Politics of Illusion* (London, 1975); I. Geiss, *Studien über Geschichte und Geschichtswissenschaft* (Frankfurt, 1972); and A. Sywottek, "Die Fischer-Kontroverse," in I. Geiss and B.-J. Wendt, eds., *Deutschland in der Weltpolitik des 19. und 20. Jahrhunderts* (Düsseldorf, 1973), 19–74.

⁴ Jürgen Kocka, "Ursachen des Nationalsozialismus," *Aus Politik und Zeitgeschichte*, June 21, 1980, 9–13. Hans-Jürgen Puhle encapsulates this argument when he attributes the rise of fascism to a political culture in "which the consequences of a delayed state formation and delayed industrialization combined closely together with the effects of the absence of bourgeois revolution and the absence of parliamentarianism to form the decisive brakes on political democratization and social emancipation"; Hans-Jürgen Puhle, *Von der Agrarkrise zum Präfaschismus* (Wiesbaden, 1972), 53.

⁵ Ralf Dahrendorf, *Society and Democracy in Germany* (Garden City, N.Y., 1967), 31–45.

first introduced by the old imperial elites and their front organizations (especially the *Bund der Landwirte* [League of Farmers]) and then, in the radically altered circumstances of republican Germany, by the fascist right.⁶ It is this failure of the economically dynamic but politically impotent *Bürgertum* to break the hold of the traditional elites on the government and forge a liberal democratic tradition on either the British or French models that constitutes, in the orthodox argument, the second major element of Germany's flawed struggle toward "modernity."

Thus the origins of German fascism, and by implication its roots elsewhere, lay in the continuation of this "flawed" development. As Winkler has argued, it is largely the power of these lingering pre-industrial traditions that explains "why [in a crisis] certain capitalist societies become fascist and others not."⁷ Kocka is even more pointed. Paraphrasing Max Horkheimer's famous injunction, he contends that "whoever does not want to talk about pre-industrial, pre-capitalist, and pre-bourgeois traditions should keep quiet about fascism."⁸

But where do such pre-industrial residues exist and how do they manifest themselves politically? All of the major exponents of the new orthodoxy have concentrated their research primarily on interest-group formation and behavior. Winkler has examined organizations representing self-employed artisans and small shopkeepers (the *gewerblicher Mittelstand* [commercial middle class]), while Puhle has focused on interest groups in the agrarian sector, and Kocka's massive collection of works deals with white-collar employees of the "new middle class."⁹ In the programs and appeals of these groups, they, along with many others, find strong, antiliberal, anti-Marxist, and often anti-Semitic positions as well as a remarkable preoccupation with corporatist notions of *Stand*, or estate, a preoccupation considered evidence of "pre-industrial, pre-capitalist, and pre-bourgeois" values. It is precisely the power and persistence of such values, these scholars argue, that distinguish the German *Mittelstand* from the middle classes of Britain, France, and the United States and ultimately made it vulnerable to the appeal of fascism.¹⁰

Over the past few years, however, this new orthodoxy has come under increasing fire from a group of British and American historians who have challenged several key assumptions, especially the acceptance of modernization theory implicit in much of this work. Attuned to the development of social history in Britain and

⁶ This model of manipulation from above is most fully elaborated in H.-U. Wehler, *Das Deutsche Kaiserreich 1871–1918* (Göttingen, 1973), esp. 96–105.

⁷ Heinrich August Winkler, "Die 'neue Linke' und der Faschismus: Zur Kritik neomarxistischer Theorien über den Nationalsozialismus," in Winkler, *Revolution, Staat, Faschismus* (Göttingen, 1978), 116.

⁸ Kocka, "Ursachen des Nationalsozialismus," 11.

⁹ Winkler's publications on this topic are extensive. Among the most important are *Mittelstand, Demokratie und Nationalsozialismus: Die politische Entwicklung von Handwerk und Kleinhandel in der Weimarer Republik* (Cologne, 1972); and "From Social Protectionism to National Socialism: The German Small-Business Movement in Comparative Perspective," *Journal of Modern History*, 48 (1976): 1–18. Hans-Jürgen Puhle's most important works on the peasantry are his *Agrarische Interessenpolitik und preussischer Konservatismus im Wilhelminischen Reich (1893–1914)* (Hannover, 1967); and *Politische Agrarbewegungen in kapitalistischen Industriegesellschaften: Deutschland, USA und Frankreich im 20. Jahrhundert* (Göttingen, 1975). Also particularly relevant are Jürgen Kocka's *Angestellte zwischen Faschismus und Demokratie. Zur politischen Sozialgeschichte der Angestellten: USA 1890–1940 im internationalen Vergleich* (Göttingen, 1977); and "Vorindustrielle Faktoren in der deutschen Industrialisierung: Industriebürokratie und 'neuer Mittelstand,'" in Michael Stürmer, ed., *Das kaiserliche Deutschland: Politik und Gesellschaft, 1870–1918* (Düsseldorf, 1970).

¹⁰ See Winkler, "Die 'neue Linke' und der Faschismus," 78–80. For their most recent reflections on the controversy, see Jürgen Kocka, "German History before Hitler: The Debate about the German Sonderweg," *Journal of Contemporary History*, 23 (1988): 3–16; and Hans-Ulrich Wehler, "Wie bürgerlich war das Deutsche Kaiserreich?" in Jürgen Kocka, ed., *Bürger und Bürgerlichkeit im 19. Jahrhundert* (Göttingen, 1987), 243–80.

France and drawing theoretical inspiration from Gramscian and Poulantzasian Marxism, the "new revisionists," as they have been dubbed, reject the "pre-industrial traditions" argument. That argument, they contend, not only vastly oversimplifies complex social and political developments but also suggests that "fascism follows logically from patterns of partial or uneven 'modernization,' which throw unreformed political institutions and 'traditional' social structures into contradiction with the 'modern' economy." In some versions of the new orthodoxy, Geoff Eley complains, fascism is effectively redefined "as a more general problem of political backwardness."¹¹

The major thrust of the new revisionist critique has been an attack on the orthodox view of Germany's "failed bourgeois revolution" and "the claim that the *Kaiserreich* somehow lacked a 'modern' political system because a 'pre-industrial power elite' managed to preserve the 'anti-democratic' bases of its power by successfully containing the bourgeoisie within the institutional and ideological structures of the traditional society."¹² German political culture was, in the orthodox view, dominated by a set of pre-industrial elites who employed a battery of manipulative strategies to mobilize a weak, insecure, and almost infinitely malleable middle class. Geoff Eley, David Blackbourn, and their Anglo-American comrades have relentlessly challenged this interpretation of political mobilization, and their research has revealed a bourgeoisie far less tractable, more politically assertive (if not liberal), and certainly more complex than in the overschematized Wehlerian depiction.¹³

The new revisionist scholarship has also challenged the significance of "pre-industrial" residues in German political culture by pointing to the wide discrepancy between the social and political rhetoric of middle-class interest groups as analyzed by Winkler, Kocka, and Puhle and more complex social realities. In a perceptive analysis of the *Mittelstand* in Imperial Germany, Blackbourn, for example, has found that horizontal and vertical differentiation increased within the middle class, especially after 1890. He has also argued that the growth of interest organizations for white-collar employees, civil servants, farmers, shopkeepers, and artisans in the period before World War I reflected a badly fragmented middle class with a wide range of divergent interests and values rather than a socially homogeneous *Stand*. Contemporary conservative claims about the existence of a *Mittelstand*, he claims, "were at odds with the facts," and conservative politicians and social theorists persisted in employing the term primarily "because of their need to deny the inevitability of class differences and social conflict." The term, in fact, the whole vocabulary of *Stand*, with its implied community of values and norms, was, in Blackbourn's view, "more prescriptive than descriptive." The *Mittelstand* was not a

¹¹ Geoff Eley, "What Produces Fascism: Preindustrial Traditions or a Crisis of a Capitalist State," *Politics and Society*, 12 (1983): 65.

¹² Geoff Eley, "Capitalism and the Wilhelmine State: Industrial Growth and Political Backwardness," in Eley, *From Unification to Nazism: Reinterpreting the German Past* (London, 1986), 44. See also Richard J. Evans, "The Myth of Germany's Missing Revolution," in Evans, *Rethinking German History: Nineteenth Century Germany and the Origins of the Third Reich* (London, 1987), 93–122; and the essays in David Blackbourn, *Populists and Patricians: Essays in Modern German History* (London, 1987).

¹³ The most sharply focused presentation of the "new revisionist" position is found in David Blackbourn and Geoff Eley, *The Peculiarities of German History: Bourgeois Society and Politics in Nineteenth Century Germany* (Oxford, 1984). That work expands their *Mythen deutscher Geschichtsschreibung. Die gescheiterte Revolution von 1848* (Frankfurt, 1980), the combative tenor of which did much to set the tone of the current debate. See also Richard Evans's introduction to *Society and Politics in Wilhelmine Germany* (London, 1978), 11–39; Eley's essay in the same collection, "The Wilhelmine Right and How It Changed," 112–35; and, of course, Eley's *Reshaping the German Right: Radical Nationalism and Political Change after Bismarck* (New Haven, Conn., 1980).

"static, traditionally minded group faced with the traumas of modernization" but a "hybrid class with fluid class boundaries."¹⁴

Similarly, Jane Caplan has argued quite plausibly that stereotypes of the *Berufsbeamtentum*, or professional civil service, as a self-contained social monolith, a *Stand*, are unrealistic oversimplifications that ignore substantial differences in qualifications, income, education, and career expectations within the civil service. Wehler's contention that civil servants from the postman to the foreign office official were bound together by a shared sense of prestige and identity derived from association with the power of the German state is, therefore, called into question.¹⁵ Following a parallel line of analysis, Robert Moeller, in his research on the Westphalian and Rhenish peasantry, has clearly demonstrated that the farmers (*Bauern*) of the Imperial and Weimar eras were hardly backward-looking, nostalgic captives of a "pre-industrial, pre-capitalist" mentality unable to adapt to the demands of an advanced capitalist society. Nor did they constitute the undifferentiated and monolithic peasantry (*Bauernstand*) frequently encountered in the literature.¹⁶

THIS, THEN, IS THE PRESENT JUNCTURE of the debate on "pre-industrial" traditions and their relationship to the rise of fascism in Germany. Wehler, Winkler, Kocka, and others associated with the new orthodoxy continue to insist that the powerful persistence of corporatist perceptions in the political culture of the Weimar Republic was symptomatic of lingering pre-industrial residues that sharply distinguished Germany's social and political development from that of Britain, France, and the United States. In contrast, the Anglo-American revisionists continue their efforts to debunk this widely accepted view by pointing out additional discrepancies between articulated ideology and social reality. As the lines of argumentation have hardened, positions have become increasingly predictable. Having charted new and exciting passages through the often-turbulent waters of modern German historiography for almost a decade, the *Sonderweg* debate has drifted into the doldrums.

To break this current impasse, I would like to suggest a new approach, one that focuses on the actual language of political discourse, what I will refer to here as the social vocabulary of everyday politics. Such an analysis, in my view, should stand at the epicenter of this debate. It is essential if we are to understand "how political mobilization occurs, how ideology is constructed and how consciousness is formed."¹⁷ Yet, curiously, neither the leading figures of the new orthodoxy nor their revisionist critics have addressed themselves in a direct or systematic way to this key ingredient of political mobilization. Indeed, in the very eye of the storm,

¹⁴ David Blackbourn, "The *Mittelstand* in German Society and Politics 1871–1914," *Social History*, 2 (1977): 409–33, esp. 432–33.

¹⁵ Jane Caplan, "The Imaginary Universality of Particular Interests: 'The Tradition' of the Civil Service in German History," *Social History*, 4 (1979): 299–317. See also Caplan's *Government without Administration: State and Civil Service in Weimar and Nazi Germany* (Oxford, 1988).

¹⁶ See Robert G. Moeller, *German Peasants and Conservative Agrarian Politics, 1914–1924* (Chapel Hill, N.C., 1986). See also his "Introduction: Locating Peasants and Lords in Modern German Historiography," and "The Economic Dimensions of Peasant Protest in the Transition from Kaiserreich to Weimar," in Moeller, ed., *Peasants and Lords in Modern Germany: Recent Contributions to Agricultural History* (Boston, 1986).

¹⁷ Eley, *Reshaping the German Right*, 11.

largely unexamined and unchallenged, lies the actual social language of political discourse itself.¹⁸

The analysis of political language in its social context is still in its infancy. The much-discussed "linguistic turn" of recent years has been focused primarily on intellectual and cultural history, but its implications extend far beyond these areas of perhaps obvious application to the remoter realms of political history and political sociology. It has already assumed an increasingly salient profile in works grappling with the formation of political consciousness and mobilization within the working classes of England and France. The potential rewards of this new sensitivity to language or "discourse" are considerable. If, as Joan Scott has recently observed, language "reveals entire systems of meaning or value—not only how people think about particular issues, but also how they understand the organization of their lives and the world," then we may, by systematically examining language, "ascertain which factors were critical in constructing the social vision through which people established, interpreted, and acted on their place in society."¹⁹

The "linguistic turn" subsumes a wide variety of theoretical and methodological positions, many of them conflicting, but, whether explicitly grounded in discourse analysis derived from literary theory or not, the new linguistically oriented studies challenge traditional paradigms of reality and representation in a fundamental way. They are not content to juxtapose rhetorical forms or linguistic constructions with actual social or economic conditions. It is precisely the space or gap between words and things, between political utterances, rhetoric, and "reality" that has traditionally occupied historians, Hayden White has argued, and that the recent literary or linguistic approaches seek to displace.²⁰ Almost all analyses of political behavior, whether Marxist or otherwise, proceed from the assumption that, as Lynn Hunt has phrased it, "the essential characteristics of politics can only be explained by their relation to a social ground."²¹ Few would dispute a "linkage between the social and the semiological," but the new focus on language does, as William Sewell points out, raise questions about "the ontological priority of economic events."²² Indeed, political language, Gareth Stedman Jones contends, cannot simply be decoded "to reach a primal and material expression of interest

¹⁸ Both Winkler and Kocka have traced the development of the relevant social terminology in their works, though in a cursory way. See Winkler, *Mittelstand, Demokratie und Nationalsozialismus*, 21–26; and Jürgen Kocka, *Unternehmensverwaltung und Angestelltenschaft* (Stuttgart, 1969), 148–96. In addition, a number of studies have focused on official Nazi vocabulary after 1933 or on the language of Nazi theoretical-ideological writings and speeches. See Lütz Winckler, *Studie zur gesellschaftlichen Funktion faschistischer Sprache* (Frankfurt, 1979), which concentrates on *Mein Kampf*; Cornelia Berning, *Die Sprache des Nationalsozialismus* (Berlin, 1961); and her *Vom 'Abstammungsnachweis' zum 'Zuchtwart.' Vokabular des Nationalsozialismus* (Berlin, 1964); Werner Betz, "The National Socialist Vocabulary," in *The Third Reich* (London, 1955); and the impressive work of Jean-Pierre Faye, *Totalitäre Sprachen* (Frankfurt, 1977). To my knowledge, there is no systematic analysis of the language of grass-roots political mobilization.

¹⁹ Joan W. Scott, "On Language, Gender, and Working Class History," *International Labor and Working-Class History*, 31 (Spring 1987): 6.

²⁰ Hayden White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore, Md., 1973), 38.

²¹ Lynn Hunt, *Politics, Culture, and Class in the French Revolution* (Berkeley, Calif., 1981), 12.

²² William H. Sewell, Jr., *Work and Revolution in France: The Language of Labor from the Old Regime to 1848* (Cambridge, 1980), 11. On the possibilities of establishing such relationships, see "Language and History," *History Workshop*, 10 (1980): 1–5. For an important but less sanguine view, see Lynn Hunt's comments on François Furet's approach to language and political action in her review of *Penser la Révolution française, History and Theory*, 20 (1981): 313–23. Sewell's remarks are found in *Work and Revolution in France*, 11.

since it is the discursive structure of political language which conceives and defines interest in the first place."²³

Obvious problems exist in elevating language to the status of an independent variable in social and political analysis, a step implicit in the logic of Stedman Jones's formulation. It is, however, precisely the effort to "decode" the political language of Imperial and Weimar Germany, linking, for example, the corporatist terminology often encountered in contemporary political discourse on "pre-industrial" social groups, or, on the other hand, dismissing the language as counterfeit if it does not mirror concrete social relations, that has led to the current stalemate between orthodox and revisionist approaches to the question of pre-industrial vestiges in German political culture. What historians must do, Stedman Jones suggests, is "study the political languages themselves," mapping out "successive languages of radicalism, liberalism, socialism . . ." and, one might add, fascism.²⁴

Examining these political languages constitutes only the beginning, for a comprehensive analysis of political discourse in any period or place would ultimately require, as Sewell has noted, "reconstructing the meanings of the words, metaphors, and rhetorical conventions that [the people or groups in question] used to think about their experiences" as well as other forms of expression and activity to determine "the symbolic content and conceptual coherence of all kinds of experiences."²⁵ Subjecting the social language of Weimar political culture to this sort of analysis is a colossal undertaking, well beyond the scope of this exploratory essay. What follows is, therefore, intended as a preliminary step, an analytic wedge to open the door to the rich potential for political analysis offered by an examination of language. The study of political discourse has not yet broken out of its working-class milieu nor has it crossed the Rhine, but its relevance to the *Sonderweg* controversy and the debate on the social origins of fascism or the related question of pre-industrial values in middle-class political and social life is considerable. Since the role of corporatist terminology has assumed a pivotal position in the current debate on "pre-industrial residues" in German political culture, I have chosen as my starting point an examination of the vocabulary employed by the political parties, interest groups, state agencies, and individuals to define social identity in the political arena. Specifically, I have directed my attention here to the forms of social address actually used in grass-roots efforts at political mobilization, through an examination of the day-to-day partisan political literature and activities—leaflets, pamphlets, posters, speeches, and meetings—for every national campaign and a great many regional and local contests from 1919 to Adolf Hitler's assumption of power in January 1933. I have focused in particular on the direct appeals of the parties to different groups of German society.²⁶

²³ Gareth Stedman Jones, *Languages of Class: Studies in English Working Class History 1832–1982* (Cambridge, 1983), 22.

²⁴ Stedman Jones, *Languages of Class*, 22.

²⁵ Sewell, *Work and Revolution*, 11.

²⁶ The massive collections of Weimar political mobilization literature in the following archives provide evidence for my arguments: Bundesarchiv Koblenz (hereafter, BA), the Geheimes Staatsarchiv preussischer Kulturbesitz Dahlem (hereafter, GStA), the Bayerisches Hauptstaatsarchiv Munich (hereafter, BHStA), the Landesarchiv Berlin (hereafter, LAB), the NSDAP Hauptarchiv (hereafter, HA), the Niedersächsisches Hauptstaatsarchiv Hannover (hereafter, NHStA), the Staatsarchiv Münster, and the Hoover Institution Archives, Weimar Republic Collection.

THE PRINTED PROPAGANDA MATERIALS, especially the ubiquitous leaflets used by the parties at the grass roots, occupy a central position in this analysis. The reliance of the Weimar parties on them is clearly evident in the hundreds of propaganda and campaign directives produced between 1919 and 1933, and they cannot be dismissed merely as "throwaway" literature. In an era before television or radio, the parties in Germany placed enormous emphasis on the drafting and distribution of such materials.²⁷ Public political discourse throughout the Weimar period was still largely dominated by the print media. Use of electronic communications, radio and film, acquired importance only after 1933 and was not really a significant factor in political mobilization strategies during the Weimar Republic. Instead, the parties relied on the distribution of leaflets, pamphlets, and posters to saturate the public with their message, while their meetings, rallies, and other public events were given prime coverage in the partisan press.

Also of tremendous importance in the political discourse of the period was what the National Socialists referred to as "propaganda of the spoken word."²⁸ Speeches delivered to local audiences at public political meetings, ceremonies, and demonstrations occupied a central place in Weimar political practice and were often coordinated with the distribution of printed matter such as flyers or placards devoted in a given theme or addressed to a particular group. The major national parties developed cadres of speakers who were made available to local affiliates for precisely such functions. Indeed, several parties developed training programs for speakers at all levels of the party hierarchy, and the National Socialist German Workers' party (NSDAP) even provided its corps of traveling speakers with sample speeches, themes, and other substantive and stylistic suggestions for addressing different audiences on different occasions.²⁹

Extensive collections of this political recruitment literature are available in the archives of both the German Federal and Democratic republics, as are detailed police reports on public partisan meetings of all sorts. As a result, it is possible to recapture the social vocabulary of political discourse in the Weimar Republic and to ascertain how the parties defined and located social constituencies, identified the issues they thought important to each of the major social, confessional, and demographic groups in German society, and to determine how these issues were presented to the different elements of the public, with what form and with what language. These activities, particularly the direct appeals to specific groups—their packaging, presentation, and vocabulary as found in leaflets, pamphlets, posters, and speeches—vividly reveal the central working assumptions of the Weimar

²⁷ The centrality of such written materials, especially leaflets and posters, in Weimar political culture is vividly reflected in the propaganda directives of the various parties but especially in National Socialist propaganda operations. See, for example, the emphasis on the drafting and distribution of such materials in the weekly propaganda *Rundschreiben* of the National Socialist German Workers' party propaganda office from 1932 in HA/14/263, 15/286–290.

²⁸ See the National Socialists' basic propaganda handbook published in early 1927, *Propaganda*, which emphasizes the importance of the spoken word in meetings, rallies, and other party ceremonies; BA, NSD 12/40.

²⁹ Beginning in 1928, the NSDAP offered a correspondence course for speakers run by Gauleiter Fritz Reinhardt of Oberbayern-Schwaben. In May 1929, Heinrich Himmler, then head of Nazi propaganda operations, ordered that at least two party members from each district (*Bezirk*) be enrolled in the course. See Reinhardt's brochure describing the school, "Rednerschule der NSDAP," in HA/70/1529. Examples of the *Redner-Material* can be found in BA, NSD 12/58. The Social Democratic party of Germany (SPD) also had a training program for speakers and provided them with "Referentenmaterial." On the origins of the SPD's program, see Robert Michels, *Political Parties: A Sociological Study of the Oligarchical Tendencies of Modern Democracy*, Eden and Cedar Paul, trans. (New York, 1962), 68–70. See also "Material für die Reichstagswahl 1932," SPD circular, June 17, 1932, in BA, NS 26/289.

parties about the major demographic and social groups in German society and how each might be recruited. Such materials provide extensive access to the social terminology of political discourse in the Weimar era. Because used on a day-to-day basis at the grass-roots level, the social nomenclature employed in such campaign and recruitment literature provides more compelling insights into the fundamental conceptualization of social groups in Weimar Germany than do the parliamentary debates, editorials in the partisan press, or broad ideological pronouncements frequently cited in traditional scholarship.

Analysis of the social categories of political mobilization and the language used to define them reveals a number of surprising and highly suggestive patterns, from which potentially significant inferences about the nature of popular politics in Weimar may be drawn. Although social standing in Germany was clearly based on a complex amalgam of factors—education, income, occupation, and family background—the parties of the Weimar era were convinced that occupational status was by far the most reliable indicator of social self-image. In a society in which social background was officially measured by father's profession, "occupation of father" (*Stand des Vaters*) required on birth certificates, occupation listed in address and, later, telephone books, and occupational titles commonly used in forms of social address—"Herr Baumeister Schmidt," "Herr Diplom Ingenieur Braun," and even in some notable cases transferred to one's spouse, as in "Frau Professor Dr. Becker"—occupational status loomed large indeed.³⁰

Partisan strategies of political mobilization and the social vocabulary of Weimar political discourse vividly reflected this deeply ingrained sensitivity to occupational status. Although the substance of their appeals differed according to a party's social orientation, targeted constituency, and ideological posture, their day-to-day campaign literature and activities reveal a striking and suggestive uniformity in the way the Weimar parties identified and then approached different social and demographic groups. From the Nazis to the left liberal German Democratic party (DDP), the bourgeois parties of the Weimar Republic sought to disaggregate German society by occupation, relentlessly directing their appeals to highly defined occupational groupings—to self-employed artisans, merchants, farmers, white-collar employees, civil servants, or workers. Indeed, appeals to middle-class *Berufsgruppen*, that is, to people in occupations that conveyed middle-class status, constituted the basic approach of all the bourgeois parties to German society throughout the Weimar period.

The content of those appeals differed, but all the bourgeois parties employed the same terminology to disaggregate and target elements of the middle-class population for political recruitment. I have identified five operational categories: first, the *Bürgertum* or *Mittelstand* in general, terms used to encompass the entire middle-class population, though the use of *Mittelstand* was more often confined to the self-employed; second, the *Mittelstand* in specific economic sectors, especially self-employed proprietors in handicrafts and retail trade (*Handwerker* and *Gewerbetreibende*); third, farmers or peasants (*Landwirte* and, far more frequently, *Bauern*). These first three groups constitute the occupational bases, both in the city and in the countryside, of what contemporary German sociologists and others referred to

³⁰ Address books for numerous communities and cities are available in local archives. For official usage, see, for example, the Reich Statistical Bureau's examination of the social backgrounds of university students, which classified them according to "occupation and occupational status of father." "Die Studierenden an den deutschen Hochschulen im Sommersemester 1929," *Vierteljahrshefte zur Statistik des Deutschen Reiches*, 39. Jahrgang 1930, Ergänzungsheft II (Berlin, 1930), 15.

as "the old middle class." The bourgeois parties also targeted the two *Berufsgruppen* of the so-called "new middle class," white-collar employees (*Angestellte*) and civil servants (*Beamte*) for recruitment.

Appeals to these last two groups were augmented by partisan events and printed matter aimed at pensioners, widows, and small investors (*Rentner*), often significantly lumped together under the term *Rentnermittelstand*. These were not abstract sociological terms formulated by the Reich Statistical Bureau or by the parties but terms enjoying wide public currency and conveying immediate—if not always unequivocal—social content to the public.³¹

Occupational appeals were almost invariably expressed in the masculine form, the parties only occasionally addressing themselves explicitly to female white-collar workers, female civil servants, or rural women. The issue of working women remained problematic for all the Weimar parties, even the Social Democrats and Communists. They tended to sidestep the issue by placing women in existing occupational categories with little sustained attention to the problems of women in the workplace. In addition, the bourgeois parties in particular clearly expected women to identify with the occupational interests of their husbands or fathers or to be mobilized by appeals focusing on what were taken to be "women's issues," especially cultural, educational, and religious matters. As a consequence, appeals to women, both those employed in the work force and those in the home, were far less prominent in Weimar campaign practice than appeals to occupational groups.³²

ALL THE MAJOR PARTIES felt it necessary to tout in their ranks for specialists to help mobilize support in the most important *Berufsgruppen*. All drafted numerous position papers addressed to specific occupational groups, held highly publicized special recruitment campaigns for them, distributed millions of occupation-specific leaflets and fliers, and published an endless stream of pamphlets explaining the party's stance on the concerns and problems of the major occupational groups. Each party considered it essential to have wide occupational representation on its list of candidates. As Gustav Stresemann reported in 1920, the German People's party (DVP) was "now experiencing in all electoral districts an extraordinary emphasis on occupational as well as local interests." In thinking about Reichstag

³¹ These terms became part of the mainstream of public usage during the nineteenth century and were adopted by increasingly widespread and powerful occupational associations to define their members. They were reinforced for civil servants by legal definitions and for white-collar employees by separate insurance organizations and plans. For the historical etymology of these terms, see the essays on *Angestellter*, *Berufsstand*, *Bürgertum*, *Mittelstand*, and others in Otto Brunner, Werner Conze, and Reinhart Koselleck, eds., *Geschichtliche Grundbegriffe. Historisches Lexikon zur politisch-sozialen Sprache in Deutschland*, 5 vols. (Stuttgart, 1972–78).

³² Despite the explosion of works devoted to women in the Weimar era, remarkably little attention has been paid to how the parties sought to mobilize female voters. Most of the literature has focused on how women voted and the position of women in the National Socialist constituency, rather than on partisan tactics and strategies for reaching the female electorate. For a sample of the most recent literature, see Helen L. Boak, "'Our Last Hope': Women's Votes for Hitler—A Reappraisal," *German Studies Review*, 12 (Fall 1989): 289–310; and her "Women in Weimar Germany: The 'Frauenfrage' and the Female Vote," in Richard Bessel and E. J. Feuchtwanger, eds., *Social Change and Political Development in Weimar Germany* (London, 1981), 155–73; Silvia Kontos, *Die partei kämpft wie ein Mann: Die Frauenpolitik der KPD in der Weimarer Republik* (Stroemfeld, 1979). See also Renate Bridenthal and Claudia Koonz, "Beyond Kinder, Küche, Kirche: Weimar Women in Politics and Work," in Renate Bridenthal, Atina Grossmann and Marion Kaplan, eds., *When Biology Became Destiny: Women in Weimar and Nazi Germany* (New York, 1984), 33–65.

Beamte!

Mit Parteibuchbonzen hat man Euer ehemals sauberes und unbestechliches Berufsbeamtentum durchsetzt. Eine maßlose Hetze hat demzufolge gegen Euch eingesetzt.

Euer verfassungsmäßiges Recht nimmt man Euch!

**Jetzt erfolgen Raubzüge
auf Euer Einkommen!**

Beamte! Lasst Euch dies nicht länger gefallen!
Das Recht ist bei Euch! Kämpft mit uns!

Postinspektor

Sprenger

M. d. R. und preuß.

Staatsrat

spricht am

Mittwoch, 11. März, 20³⁰ Uhr

in einer

im Kyffhäuserhaus, Turnerstr.

öffentl. Beamtenversammlung

über

Beamter und Nationalsozialismus

Erscheint in Massen!

Eintritt 50 Pfg.

Aussprache!

Für Juden verboten!

Kartenvorverkauf: Parteigeschäftsstelle, Härtelestrasse 23, I

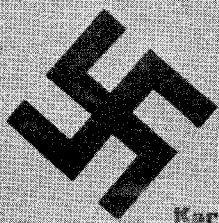


FIGURE 1: "Civil Servants! You've seen your once pure and incorruptible career civil service saturated with party book bosses. As a result, a boundless agitation has been unleashed against you! Now a raid on your income . . . Civil Servants! Don't stand for it any more! Justice is on your side! Join our struggle! Postal Inspector Sprenger . . . will address a public meeting for civil servants on 'The Civil Servant and National Socialism.'" From the Bayerisches Hauptstaatsarchiv (BHStA), Plakatsammlung 10414, A1.

candidates, the people "want[ed] individuals [who were] from the district itself and members of the productive estates [*schaffenden Stände*]." ³³

Thus each party sought to convince its targeted occupational audience that it alone understood the historical development, special economic needs, and social virtues of the occupational position (*Berufsstand*) in question and that it alone had protected and advanced the interests of that "occupational estate." The pamphlet series of the liberal but right of center DVP, the *Flugschriften der Deutschen Volkspartei*, offers typical examples of this attention to occupation in its 1924 special issues, "Civil Service Policies of the DVP," "White-Collar Issues in the Reichstag," "Agriculture and Party Politics," and "The *Mittelstandspolitik* of the DVP." The left liberal DDP followed suit, devoting issues of its *Materialien zur Demokratischen Politik* to "DDP and the Commercial *Mittelstand*," "Civil Service Questions," and "DDP and Agriculture." ³⁴ Nor was this occupational emphasis limited to printed matter. In 1930, over half of the NSDAP's cadre of major national speakers listed such themes as their specialties. ³⁵

The most readily quantifiable evidence of this occupational orientation in Weimar political culture is found in party leaflets. During the campaigns of 1932, for example, occupational appeals represented by far the largest category of leaflets drafted by the propaganda office (*Reichspropagandaleitung*) of the NSDAP for nationwide distribution. Over one-third of the approximately 190 leaflets drawn up at party headquarters were addressed explicitly to farmers, civil servants, workers, or shopkeepers. In fact, this figure significantly understates the number of printed propaganda materials aimed at specific occupations, since most of the leaflets attacking rival parties, defending the NSDAP against partisan "lies," or dealing with specific economic or social issues were, upon an even cursory reading of the text, targeted at individual occupations. ³⁶

Considerable evidence for the extensive distribution of these occupation-specific leaflets is available in virtually every regional collection of printed propaganda materials and in the police reports of grass-roots political mobilization. Perhaps the best quantitative evidence can be found in records of a questionnaire concerning local recruitment activities dispatched to county propaganda operatives by the Nazi leadership of Gau South Hannover-Braunschweig in preparation for the upcoming

³³ See Gustav Stresemann's letter to Rudolf von Campe, February 6, 1920, cited in Wolfgang Hartenstein, *Die Anfänge der Deutschen Volkspartei 1918–1920* (Düsseldorf, 1962), 213.

³⁴ See, for example, "Fragen der Kleinrentner—DVP und Aufwertung," "Angestelltenfragen im Reichstag," "Mittelstandspolitik der DVP," "Landwirtschaft und Parteipolitik," and "Beamtenpolitik der DVP," Folgen 49, 51, 55, 57, and 58 in the DVP's *Flugschriften der Deutschen Volkspartei* (Berlin, 1924); and the DDP's "DDP und Landwirtschaft," "DDP und gewerblicher Mittelstand," and "Beamtenfragen," Folgen 84, 88, and 93 of the party's pamphlet series *Materialien zur demokratischen Politik*, also from 1924.

³⁵ The largest single specialty was agricultural issues (*Bauernfragen*), followed by civil-service (*Beamtenfragen*) and working-class issues. The list is found in HA/Reel 24A/Folder 1758.

³⁶ Drafts of these leaflets are in HA/15/286–290. Of the 186 leaflets intended for national distribution, 35 percent appealed to specific occupational groups; 28 percent focused on specific themes or issues; 11 percent attacked individual parties or appealed to their supporters (most were clearly targeted for workers in the SPD); 8 percent dealt with Hitler, Hindenburg, and Thälmann, candidates in the spring presidential campaigns; 7 percent were addressed to veterans and *Rentner*; 5 percent were directed at women; and 4 percent dealt with religion (usually, appeals to Catholics). In the occupation-specific category, 35 percent of the appeals were aimed at civil servants, 30 percent at workers, and 27 percent at farmers. This distribution was roughly repeated in the official *Partei-Korrespondenz* during the elections of 1932, whose articles were intended to serve as models for public propaganda appeals. Between March and December, 101 items dealing with occupational and demographic groups appeared. Of these, 22 percent dealt with farmers; 16 percent with *Rentner*; 15 percent with religion; 11 percent with the *Mittelstand*; 10 percent with civil servants; 10 percent with workers; 8 percent with youth; 7 percent with women; and roughly 1 percent each with white-collar employees and professionals. *Nationalsozialistische Partei-Korrespondenz*, Folgen 37–286, located in BA, NSD 13/7–11.

fall Reichstag campaign. Leaflets, as usual, occupied a central position in the party's plans for "propaganda through pictures and the written word." Local Nazi groups were offered a selection of eight different types of leaflets to order, and the categories of choice suggest once again the important role of occupation. They were "1) General, 2) Peasants, 3) Workers, 4) Civil Servants, 5) *Mittelstand*, 6) Women, 7) Pensioners [*Rentner*], and 8) Zentrum [Catholic Center party]." The returned questionnaires, from over sixty local affiliates (*Ortsgruppen*), revealed that 51 percent of the leaflets actually ordered for the campaign were occupation-specific.³⁷

While some leaflets, especially those of a general nature, were passed out indiscriminately at the polls, tossed from campaign trucks, or, in at least one spectacular case, hurled from an airplane,³⁸ occupation-specific materials were treated differently. Local affiliates of the NSDAP were expected to use local address books and other available intelligence to determine the occupations present in each household and to ensure that leaflets and other occupation-specific materials were delivered accordingly. In Mannheim, for example, block leaders of the party were to establish a filing system on the inhabitants of their area, using the following categories to subdivide the population for political mobilization: "1) workers (private, state, and communal), 2) white-collar employees [in same sectors], 3) civil servants (state and local), 4) artisans, 5) retail merchants [*Einzelhändler*] and tavernkeepers." Moreover, as the Mannheim party's directive to its operatives made clear, "it is irrelevant whether the resident is currently on the dole, retired, or employed." Current economic status, in other words, was less significant, hard-headed Nazi propaganda officials believed, than the recipient's sense of occupational identity.³⁹

In many cases, the residential delivery of such occupation-specific materials was followed up by a personal visit, *Hauspropaganda*, as the Nazis called it, or by an invitation to a special National Socialist meeting devoted to the problems of the relevant occupational group.⁴⁰ The NSDAP's county organization in Insterburg, East Prussia, for example, issued personal invitations to all local civil servants and their families for a special meeting (*Beamtenversammlung*) in May 1932. Typically, the party's Gau leadership provided a civil servant as the speaker for the occasion, an *Obersekretär* in the East Prussian administration. The Insterburg Nazis also held special meetings for farmers, artisans, and workers in the same period.⁴¹ Across the country, police officials in the Rhineland reported that the NSDAP had held similar

³⁷ See "Fragebogen für die Propaganda durch Bild und Schrift," sent by the NSDAP-Propagandaabteilung Gau Süd-Hannover-Braunschweig to all county directors (*Kreisleiter*) on October 3, 1932. Responses were to be returned in four days. NHStA, 310/I, B/nr. 26.

³⁸ Descriptions of Brown Shirt (SA) units tossing fistfuls of leaflets from speeding trucks are commonplace in the case-study literature. The airplane incident occurred over Berlin during the Prussian Landtag campaign in April 1932; see LAB, Rep. 58/nr. 1564.

³⁹ "Kleinarbeit-Aktion der Ortsgruppe Mannheim für beide Wahlkämpfe zur Reichspräsidentenwahl 1932," BA, NS22/1044.

⁴⁰ Nazi propaganda operatives in Nienburg, a rural county in Lower Saxony, reported, for example, that "all meetings here have been announced with cards personally addressed to everyone. Here in the countryside personal invitations are the most effective"; Tätigkeitsbericht, Kreisleitung Nienburg, from February 5, 1932, in NHStA, Hann. 310/I, B11/II. See also "Hauspropaganda" in "Richtlinien der Propaganda-Abteilung, NSDAP Gau Süd-Hannover-Braunschweig," NHStA, Hann. 310/I, B5.

⁴¹ See the *Beschaffenheitsbericht* of the Kreisleitung Insterburg for the months October–December 1931 and the correspondence between the county propaganda section and the *Gauleitung* from March 29, 1932, and May 20, 1932. Gau propaganda officials also offered a former Communist Landtag deputy to address a special meeting for workers and the unemployed planned for later in May 1932; GStA, HA XX, Rep. 240/C51.

meetings for civil servants in several Rhenish cities and towns in the spring of 1932, along with special party events for farmers, vintners, and workers.⁴² Indeed, such occupation-specific meetings were staples of National Socialist propaganda throughout Germany.⁴³

The Nazis also employed form letters for direct mailing addressed to individuals of specific occupational and demographic groups. The letters were intended to look like personal communications written by a National Socialist from the recipient's own occupational background. One such letter for farmers typically began with the informal "Lieber Herr ———," and then proceeded to discuss "the misery of our *Berufsstand*," while another, targeted for working-class youth, opened with "Lieber Kampfgenosse ———, Millions of young workers are starving in Germany." These techniques, centering on the notion of *Beruf* or *Berufsstand*, were basic to National Socialist mobilization and recruitment efforts throughout the Weimar period.⁴⁴

The NSDAP was certainly more thorough and more systematic than its adversaries in the Weimar party system, but the National Socialist attention to *Beruf* in its conceptualization of German society was hardly unique. All the parties, despite significant differences in ideological orientation, assumed the centrality of occupation in the formation of social identity. In fact, disaggregating the population by occupation was so axiomatic in Weimar political practice that party strategists rarely ever bothered to comment explicitly on the importance of *Beruf* in their internal memoranda. A circular to DDP operatives anticipating the 1928 Reichstag elections does, however, provide a revealing glimpse into the thinking of the parties. Written by Anton Erkelenz, the circular emphasized the importance of "mailing special leaflets . . . aimed at specific occupational strata [*Berufsschichten*] whose addresses one can determine from address books and electoral lists." After breaking down the population according to such categories as "entrepreneurs, civil servants, *Mittelständler* of all sorts, teachers, etc.," materials could be sent "that go into their special wishes and interests. It is obvious that this is extremely desirable. For the average German, according to experience, is less interested in the great political ideas than in the details, trifles [*Kleinigkeiten*] and narrower interests of his own special sphere."⁴⁵

Other parties might have disputed Erkelenz's assumptions about the role of "great political ideas," but his stress on the importance of occupation would have raised no eyebrows.

A typical campaign directive of the Catholic Center party to its local organiza-

⁴² Authorities in Cologne, Düsseldorf, Oberhausen, Wuppertal, Duisburg, and Remscheid-Lennep all reported special Nazi *Beamtenversammlungen* in early 1932. See report of the Polizei-Präsidium Köln to the Regierungspräsident-Koblenz, January 14, 1932, and the report of the Regierungspräsident Düsseldorf to the Oberpräsident April 14, 1932, both in Landesarchiv Koblenz (hereafter, LAK), 403/16761. See also the Report of the Oberpräsident of Rheinprovinz to the Regierungspräsidenten der Provinz, January 3, 1932, and January 29, 1932, for National Socialist activities among farmers and vintners; Hauptstaatsarchiv Düsseldorf (hereafter, HStAD), Regierung Aachen, 22984.

⁴³ The following request from Nazi grass-roots propaganda operatives was typical: "Two special meetings for handicrafts, trade, and commerce [*Handwerk, Handel und Gewerbe*] must at all costs be held in Sulingen and Diepholz, since many votes can be had from artisan circles . . . I therefore request that Party Comrade Schroeder, the syndic of the Chamber of Handicrafts, be made available [to us] for two days as speaker"; Kreisleitung Sulingen to the Gauleitung, Propaganda-Abteilung, Gau Süd-Hannover-Braunschweig, October 17, 1932, NHStA, Hann. 310/I, B24/I.

⁴⁴ The form letters, drafted for the second presidential election of 1932, are found in GStA, HA XX, Rep. 240.

⁴⁵ Anton Erkelenz, "Wie gewinnen wir die Reichstagswahlen," DDP publication, no date but drafted in anticipation of elections in 1928.



FIGURE 2: "We peasants are throwing out the manure. We're voting List 2: National Socialists." From the Bundesarchiv (BAK), Plakatsammlung 2/42/65.

tions during the summer Reichstag campaign of 1932 listed eight leaflets to be distributed during the next ten days, one each addressed to “the peasant population,” the *Mittelstand*, “workers and white-collar employees,” “the unemployed,” “women,” “youth,” and “the politically uncommitted,” while a representative DVP campaign memorandum from the same period divides the public into seven very similar groups: *Arbeitnehmer*, a term encompassing both blue-collar and white-collar workers, peasants, civil servants, the commercial *Mittelstand*, *Rentner*, women, and youth.⁴⁶ Given this extraordinary focus on occupational groupings, it is hardly surprising that when the Regierungspräsident of the Rhine Province ordered the regional and local police of his districts to investigate National Socialist mobilization activities in their locales, he directed them to report on Nazi “recruitment efforts among civil servants, the *Mittelstand*, workers, farmers, and the police.”⁴⁷

THE PARTIES OF THE BOURGEOIS CENTER AND RIGHT also systematically linked these occupational terms with the imagery of *Stand*, or estate, in an effort to exploit the lingering corporatist aspects of occupational status. Campaign appeals were at times addressed to the “peasant estate” (*Bauernstand*), the “civil service estate” (*Beamtenstand*), and, in a tortured but revealing extension of that mentality, to the “white-collar estate” (*Angestelltenstand*). Significantly, even the Marxist parties, while rejecting such corporatist terminology, were no less occupationally oriented when addressing middle-class audiences. Both the Social Democrats (SPD) and Communists (KPD) appealed explicitly to *Angestellten*, *Beamten*, *Bauern*, and even self-employed proprietors (*Gewerbetreibende*). Although both emphasized the need for solidarity among all *Arbeitnehmer* and a common front of the exploited with the small shopkeepers and family farmers, the KPD and SPD were in agreement that the most effective means of addressing these middle-class groups was through appeals to occupational categories.⁴⁸ Such appeals were supplemented by campaign literature addressed to Protestants, Catholics, women, youth, and the elderly—the major confessional and demographic components of German society—but the social vocabulary of politics in the Weimar Republic was unquestionably dominated by *Berufsstand*.

In addition to formulating these appeals, the parties of the Weimar Republic also sought to establish their position along the traditional nineteenth-century lines of class or religion.⁴⁹ While addressing specific occupational groups, a tactic that tended to emphasize conflicts of interest within the *Mittelstand*, the bourgeois parties also endeavored to generate a sense of middle-class unity by attacking, with varying degrees of radicalism, the Marxist left and embracing the cause of German nationalism. Indeed, the intent of most campaign literature or activities directed at

⁴⁶ See the Zentrum's Rundschreiben nr. 7, July 14, 1932, in BA, NS26,289. For the DVP material, see “Entwürfe für Inserate,” in BA, ZgS. 1, 42/8(3).

⁴⁷ See Regierungspräsident's instructions of June 3, 1930, to all police presidiums, Landräte, and police administrations in his province; LAK, 403/16740.

⁴⁸ See, for example, the SPD's Rundschreiben of July 1, 1932, to all SPD district leaders, which lists eleven special leaflets to be made available in the upcoming campaign. These were addressed to civil servants, white-collar employees, farmers, *Mittelstand*, small-holders, industrial workers, miners, the unemployed, sports enthusiasts (*Sportler*), youth, and women; BA, NS26,289. See also “Arbeiter, Angestellte, Beamte, Werktätige in Stadt und Land,” “An die deutsche Beamenschaft,” “An die Lehrerschaft,” and “Zehn Fragen der Bauern—Zehn Antworten der Kommunisten,” KPD leaflets, 1924, in GStA, HA XII/IV, nrs. 90–91.

⁴⁹ For the nature and history of these divisions in German political culture, see Thomas Childers, *The Nazi Voter: The Social Foundations of Fascism in Germany, 1918–1933* (Chapel Hill, N.C., 1983), 15–49.

the *Mittelstand* or *Bürgertum* as a whole was to establish the credentials of the party in question as a stalwart defender of middle-class interests and values, especially against the Marxist threat.

This aim was most obvious in the appeals of the conservative German National People's party (DNVP), the liberal but right-of-center German People's party (DVP), and the National Socialists but also played a prominent role in the appeals of the more progressive left liberal German Democratic party (DDP) and the Catholic Center party. In a typical DDP leaflet addressed to the "German middle class" in 1924, for example, the party warned the bourgeois electorate about the Socialists and Communists "who want to ruin the *Mittelstand*" and went on to claim credit for frustrating repeated Marxist efforts to expropriate the middle class since the revolution of 1918.⁵⁰

The same concerns were expressed in mobilization efforts directed at the self-employed artisans and merchants of the old middle class, supplemented by an avowed concern for the special problems of small business. The NSDAP was certainly the most aggressive advocate of the small businessman, attacking the department stores, consumer cooperatives, the banks, the stock market, and big business in general. "For years," one typical Nazi appeal from 1930 declared, "the *gewerbliche Mittelstand* has fought a desperate battle against excessively powerful great concerns and trusts which are supported by funds from the big banks."⁵¹ "The middle-class parties pledged to save the *Mittelstand* from destruction, but it is rapidly nearing its utter demise . . . The parties for the salvation of small business promise to help the small craftsman, the shopkeeper, and merchant, but with their aid, the large department stores spring up and strangle hundreds of thousands of self-employed businessmen." As a result of this process of "alienation and expropriation," the Nazis characteristically warned, the *Mittelstand* was approaching "the end of its position in the state, indeed, of its existence as a *Stand*."⁵²

Although the other bourgeois parties, especially the liberals, were far less willing to condemn big business and the banks or call for the creation of some form of corporatist economic and political structure as did the Nazis and a host of *mittelständisch* special-interest parties, all came to speak the language of *Beruf*. Emphasis differed from party to party but the idiom or imagery of *Berufsstand* flowed through appeals with titles such as "Handwerk und Gewerbe und die völkische Bewegung" (NSDAP, 1924); "Das Handwerk und die DNVP" (DNVP, 1924); "Kaufmännische und gewerblicher Mittelstand" (DVP, 1924); "Gewerbetreibende, Handwerker!" (NSDAP, 1928); "Der Einzelhandel am 14. September" (DVP, 1930); "An alle Handwerker und Kaufleute!" (DSP/DDP, 1930); "Handwerker, Gewerbetreibende, Einzelhändler. Für Mittelstandspolitik" (NSDAP, 1932).⁵³ Significantly, in virtually none of these leaflets or the hundreds of others addressed to these same groups can a hint be found that important differences in income, background, or expectations might separate the successful owner of a fashionable boutique in an affluent neighborhood from the cigarette vendor at a railway station kiosk. A sense of occupational solidarity was apparently expected to transcend such distinctions.

The same sociological assumptions are also very much in evidence in campaign appeals directed to German farmers. All the parties actively engaged in recruiting

⁵⁰ "Deutscher Mittelstand!" *Berliner Tagesblatt*, May 3, 1924.

⁵¹ "Wer schützt dich, Mittelstand?" *Der Angriff*, April 30, 1928.

⁵² "Das Gericht vom 14. September," *Völkischer Beobachter*, September 10, 1930.

⁵³ See Childers, *Nazi Voter*, 64–79, 142–59, and 211–24.

rural support sought to appeal to an assumed solidarity by focusing on “threats to the German *Bauernstand*” from hostile social and political interests: the Marxists, the bankers, the Jews, and the urban liberals. “Whether estate owner or small proprietor, both are threatened by the anti-farm policies of the black-red-yellow [pro-republican] parties,” the DNVP charged in a typical 1924 leaflet. “If you don’t cast your vote for the Nationalists [conservatives], then you can’t be surprised if the Jewish, consumer viewpoint wins the upper hand and leads to the ruin of agriculture.”⁵⁴ Although the Nazis and the plethora of small regional and peasants’ parties were far quicker to focus on the frustrations of the small family farmer, they too employed the notion of a *Bauernstand* or *Bauerntum* (peasantry) that sought to join all farmers in occupational solidarity against external social enemies.⁵⁵

Among the various groups identified and targeted for mobilization by the Weimar parties, none was treated as a more monolithic social entity than the civil service. Appeals to civil servants, whether from the NSDAP or DDP, treated the career civil service (*Berufsbeamtentum*) as an undifferentiated occupational estate. Each emphasized the elitist traditions of the career civil service, seeking to bridge the gaps between its ranks by evocations of a traditional, homogenizing social ethos. Only the Marxist parties sought to break up this ostensible monolith, addressing campaign literature to lower-ranking civil servants and attempting to establish points of linkage between government officials and other employees.⁵⁶ Characteristic of the non-Marxist mobilization efforts among civil servants is the DVP appeal in 1924 reminding public officials that on the question of “democratization” the DVP “stands in sharp contrast to the Social Democrats who want to treat civil servants . . . like every other private employee.” The DVP stood firmly against “the integration of civil servants into the great front of *Arbeitnehmer*.” If the Social Democrats had their way, the DVP typically contended, “the professional civil servant would hang suspended in constant danger of expulsion by alien elements.”⁵⁷

Even the National Socialists emphasized the elitist, corporatist traditions of the *Berufsbeamtentum*, decrying the “steady deprivation of civil service rights.” Indeed, the Nazis were not content simply to preserve existing privileges and benefits for the civil service, their mobilization appeals stressed, they would do everything they could to “reestablish all the rights that have been taken from the *Beamtentum*.”⁵⁸ Nazi propaganda aimed at civil servants also unceasingly lamented the erosion of civil service status under the “Weimar system,” an erosion produced, the NSDAP

⁵⁴ “Reichstagswahl-Preussenwahl, ein letztes Wort,” *Kreuz-Zeitung*, December 7, 1924.

⁵⁵ For a sample of the small, peasant-oriented parties, see “Wahlaufuf der Christlich-Nationalen Bauern- und Landvolk-Partei,” and “Wahlaufuf der Deutschen Bauernpartei,” in *Reichstags-Handbuch IV. Wahlperiode 1928* (Berlin, 1928), 188–90 and 190–92 respectively; or “Der Bayerischer Bauern- und Mittelstandsbund ruft auf!” leaflet of that party, 1928, BHStA, Abtg. V, Flugblattsammlung, 59, F64. For an example of the NSDAP’s appeals to peasants, see “Nationalsozialismus und Bauerntum,” *Nationalsozialistische Partei-Korrespondenz*, March 1, 1932; and “Deutscher Bauer, wer ist schuld an Deinem heutigen Elend?”; “Wer allein bringt dem deutschen Bauern die Errettung aus diesen Gefahren?”; “Bauern, Ihr werdet enteignet!”; and “Der Bauer verliert Haus und Hof,” Nazi leaflets, 1930, in BHStA, Abtg. V, Flugblattsammlung, 59, F11, NS1930.

⁵⁶ See, for example, “Wen soll die Beamten wählen?” June 4, 1920; “Beamte und Reichstagswahl,” May 15, 1924; “Beamte gehen zur Wahl,” September 13, 1930; and perhaps most revealing, “Beamte in der Volksfront,” November 3, 1932, all in *Vorwärts*. For the typical Communist appeal, see “An die untere und mittlere Beamtenschaft,” and “An die deutsche Beamtenschaft,” KPD leaflets, 1924, GStA, HA XII/IV nrs. 90–91.

⁵⁷ “Beamtenschaft und Reichstagswahl,” *Berliner Stimmen, Zeitschrift für Politik. Nachrichtenblatt der DVP im Wahlkreisverband Berlin*, nr. 19, 1924.

⁵⁸ “Armee und Beamtentum, zwei Säulen des kommenden Staates,” *Völkischer Beobachter*, June 15, 1932.

charged, by a torrent of unqualified political appointees (*Parteibuchbeamten*) who entered state service after the November revolution. This "democratization of the civil service," as the Social Democrats and liberals called it, had introduced *Standesungehörige* (outsiders), the Nazis claimed, who had little understanding of and no commitment to the traditions of "honor and duty" that had formerly characterized both "the *Beamten- und Offizierstand* [civil service and officer corps]." ⁵⁹ In taking this stance, the NSDAP had adopted a rhetorical position similar to that of the largest civil service union, the German Civil Servants' League (DBB). To officials who had experienced the shocks of "democratization" and the alleged influx of political appointees after 1918, the civil service layoffs of 1923–1924, and the cuts in pay, pensions, and benefits under Heinrich Brüning, such appeals to professional solidarity may have struck a responsive chord. ⁶⁰

Despite considerable encouragement from the nonsocialist white-collar unions, attempts by the bourgeois parties to establish a shared occupational identity, not to mention corporatist solidarity, for the socially heterogeneous white-collar labor force were far less extensive and convincing than their appeals to the groups discussed above. Certainly, the non-Marxist white-collar unions were determined to preserve a social and professional status segregated from and elevated above blue-collar labor, insisting that the German *Angestelltenschaft* (white-collar employees) represented a distinct social estate or *Stand* with its own unique economic and spiritual role in politics and society. In keeping with this view, both the liberal League of White-Collar Employees (GdA) and the conservative German National League of Commercial Employees (DHV) endorsed the maintenance of separate social agencies for white-collar employees. These organizations, such as the white-collar insurance and health administrations, perpetuated the gulf between manual and non-manual labor and were intended by the GdA and DHV to do just that. For that reason, they were vigorously opposed by the socialist white-collar union, Allgemeiner freier Angestelltenbund (AFA-Bund). ⁶¹ It was symptomatic of this corporatist orientation that when both the conservative and liberal white-collar associations complained bitterly about high rates of white-collar unemployment, falling real income, and a general trend toward "proletarianization" throughout the Weimar period, they concluded that "the social position of the *Stand* is not what it was before the war." ⁶²

While the bourgeois parties developed a clear, if oversimplified sociological picture of shopkeepers, self-employed artisans, farmers, and civil servants—groups with some claim to a corporatist heritage—they encountered considerable difficulty

⁵⁹ "Beamtentum und Nationalsozialismus," *Nationalsozialistische Bibliothek*, 30 (1931): esp. 28–31. On Nazi appeals to civil servants, see Jane Caplan, "Speaking the Right Language: The Nazi Party and the Civil Service Vote in the Weimar Republic," in Thomas Childers, ed., *The Formation of the Nazi Constituency 1919–1933* (London, 1986), 182–201.

⁶⁰ For the best analyses of the position of civil servants in the early Weimar years, see the works of Andreas Kunz, especially "Stand versus Klasse. Beamtschaft und Gewerkschaften im Konflikt um den Personalabbau 1923/24," *Geschichte und Gesellschaft*, 8 (1982): 55–86; "Verteilungskampf oder Interessenkonsensus? Einkommensentwicklung und Sozialverhalten von Arbeitnehmergruppen in der Inflationszeit 1914 bis 1924," in Gerald D. Feldman, et al., eds., *Die deutsche Inflation: Eine Zwischenbilanz* (Berlin, 1982), 347–84; as well as Andreas Kunz, *Civil Servants and the Politics of Inflation in Germany 1914–1924* (Berlin, 1986). For the trials of the *Beamtentum* in the later Weimar era, particularly the relationship of civil servants to the NSDAP, see Caplan, *Government without Administration*.

⁶¹ See Fritz W. Fischer, "Die Angestellten, ihre Bewegung und ihre Ideologien" (Doctoral dissertation, Heidelberg, 1931); and Carl Dreyfuss, "Beruf und Ideologie der Angestellten" (Doctoral dissertation, Munich, 1933).

⁶² "Wo stehen die Angestellten?" *Angestelltenstimme. Blätter zur Reichstagswahl 1928*, nr. 1, May 21, 1928.

in formulating a similarly pristine vision of white-collar employees. Appeals to *Angestellten* were, in fact, few and far between in the recruitment literature of the various parties. In the National Socialist campaigns of 1932, for example, a year in which the Nazi propaganda machine systematically canvassed virtually every conceivable social and demographic constituency, appeals to civil servants outnumbered those to white-collar employees by a surprising ten to one.⁶³ Although the NSDAP was never able to develop a clearly articulated appeal to white-collar employees, the party did attempt to make of the *Angestelltenschaft* the *Angestelltenstand*, as did the conservative DNVP, but with very mixed results. "Once the *Angestelltenschaft* was a proud occupational estate," the Nazis observed in 1932, but no longer. "Marxism coined the term 'stiff collar proletariat' not to help the white-collar employee but—after the proletarianization of artisans into industrial slaves—to fit yet another productive *Stand* into the gray army of the nameless industrial proletariat."⁶⁴ The DNVP concurred, warning white-collar employees that "Marxist party and union politics" could not improve their position. If the *Angestellten* wanted to increase their economic power and political influence, the conservatives asserted, "they must engage in *Stand*-oriented politics and liberate themselves from all union Marxism."⁶⁵ The fact that the *Angestelltenschaft* lacked a corporatist past, that most white-collar employees lived in large urban areas, that approximately one-quarter of them sprang from working-class backgrounds, and that one-third were women greatly complicated the propagandistic tasks of the bourgeois parties and made the sort of corporatist approach to white-collar labor found in the appeals to peasants, artisans, and civil servants virtually impossible.⁶⁶

Since the middle-class parties made few efforts to mobilize a working-class constituency and since the social vocabulary of the Marxist parties does not bear directly on the problem posed at the outset of this essay, appeals to workers will not be examined in any detail here. Nonetheless, a distinct social language of working-class politics did most certainly exist, and important differences in nomenclature are obvious as soon as one crosses the great social divide of German politics. The bourgeois parties, for example, steadfastly refused to speak of an *Arbeiterklasse* (working class) and referred instead to the *Arbeiterschaft* (literally, workership or working people). The Nazis, who did attempt to mobilize a working-class constituency, sometimes employed both terms but clearly favored *Arbeitertum* (workerdom), which in 1931 became the title of the official publication of the party's factory organization, the National Socialist Factory Cell Organizations (NSBO). The NSDAP also self-consciously avoided *die Werktätigen* (laborers) or *die werktätigen Massen* (laboring masses), terms favored by the Communists, preferring instead constructions such as *das schaffende Volk* (the productive people).

The Social Democrats, for their part, while emphasizing class solidarity within the *Arbeiterklasse* and among all *Arbeitnehmer*, displayed a pronounced tendency to disaggregate their working-class constituency by economic sector, appealing to workers in textiles, coal mining, machine working, the chemical industry, and construction. It might also be noted in passing that after 1928 the KPD displayed a striking tendency to borrow and adapt terms from the Nazis. Ernst Thälmann, the KPD's general secretary, became at times the "Führer," and the Nazi fondness

⁶³ This ratio holds across all the collections of campaign literature consulted.

⁶⁴ "Angestellte, es geht Euch an!" NSDAP leaflet, 1932, HA/Reel 15/Folder 289.

⁶⁵ DNVP, *Deutsche Angestellten-Zeitung. Blätter zur Reichstagswahl 1932*, July 1932, BA, ZgS. 1, 44/9.

⁶⁶ See Thomas Childers, "National Socialism and the New Middle Class," in Reinhard Mann, ed., *Die Nationalsozialisten: Analysen faschistischer Bewegungen* (Stuttgart, 1980), 19–33.

for variations of *Volk* surfaced in Communist formulations such as *Arbeitervolk*. It is perhaps significant, though, that the imagery of *Stand*, pervasive in the language of bourgeois politics, rarely appeared in appeals aimed at working-class audiences, even in the National Socialist mobilization efforts. Only in the formulations of the Christian trade unions are occasional references to the *Arbeiterstand* found.⁶⁷

Without survey data, it is impossible to determine with any degree of certainty the impact of political appeals on the groups for which they were targeted. It is, however, absolutely clear that all the Weimar parties were convinced that an occupation-specific approach was the best strategy for organizing their social appeals and mobilizing important groups in the middle-class population. It allowed them to appeal vertically across income levels, addressing civil servants regardless of rank and shopkeepers regardless of neighborhood. Perhaps most important, it enabled them to take advantage of the well-organized and active occupational associations that played a pivotal role in the social and political life of virtually every German community. For the traditional bourgeois parties, not to mention the small, special-interest parties with relatively underdeveloped grass-roots organizations, this may have been a significant consideration.⁶⁸

For the liberals, in particular, however, such appeals posed a serious, if rarely articulated, dilemma. Neither the DVP nor the DDP embraced the corporatist political and economic solutions espoused by the right and neither indulged in the romantic flights of nostalgia for the guilds or the life pastoral that appeared in the National Socialist and conservative mobilization efforts. For the liberals, the language of *Berufsstand* may have served as a convenient tactical device to mobilize support across a badly fragmented middle class that could not be attracted by appeals emphasizing liberal notions of "the free economy" (DVP) or "economic democracy" (DDP). Saving the *Mittelstand* or *Berufsbeamtentum* or *Kleingewerbe* (small shopkeepers) from socialism would then provide the various subgroups of the middle class with both a positive and negative sense of identity and provide liberalism with an attractive political mission. But there were hazards in such a strategy. "We are in danger of seeing the idea of the people [*Volksgedanke*] overwhelmed by the idea of occupation [*Berufsgedanke*]," the DDP warned in 1930. German political culture, the Democrats complained, was "unsurpassed in the organization of the occupational estate or economic stratum but pathetic in . . . fitting the self into a conception of the general welfare." Moreover, by adopting the forms without the substance implied by such corporatist vocabulary, the liberals

⁶⁷ See, for example, "Wir kämpfen für die Rechte des schaffenden Volkes," in *Nationalsozialistische Partei-Korrespondenz*, October 4, 1932, or the NSBO's *Deutscher Arbeiter. Monatszeitung für das Schaffende Volk*, in BA, NSD, 51/1. For good examples of Communist terminology, see "Letzter Appell an alle werktätigen Wähler," September 12, 1932, and "Nationalsozialismus Todfeind der Arbeiterklasse," July 8, 1932, both in *Die Rote Fahne*. For an example of the impact of Nazi language on the KPD, see "Hakenkreuz gegen Deutschlands Arbeitervolk," *Die Rote Fahne*, July 28, 1932, and "Heute spricht unser Führer Gen. Thälmann," *Die Rote Fahne*, September 12, 1930. The Social Democratic tendency to disaggregate their working-class constituency by economic sector is vividly demonstrated in the following set of appeals published in *Vorwärts* during 1924, "An die Textilarbeiter" (December 3), "Wahltag und Metallarbeiter" (April 27); "Die Maschinisten und Heizer zur Reichstagswahl" (April 24); and "Bauarbeiter wehren sich" (October 13). The Christian trade union's newspaper for coal miners, *Der Bergknappe*, bore the subtitle "Für wirtschaftliche und geistige Hebung des Bergarbeiterstandes."

⁶⁸ For the role of these local associations, see Rudy Koshar, *Social Life, Local Politics, and Nazism: Bourgeois Marburg 1880–1935* (Chapel Hill, N.C., 1986). Zdenek Zofka describes the great importance to the Nazis of winning influential opinion leaders within local occupational associations in his *Die Ausbreitung des Nationalsozialismus auf dem Lande. Eine regionale Fallstudie zur politischen Einstellung der Landbevölkerung in der Zeit des Aufstiegs und der Machtergreifung der NSDAP 1928–1936* (Munich, 1979); and his "Between Bauernbund and National Socialism: The Political Reorientation of the Peasants in the Final Phase of the Weimar Republic," in Childers, *Formation of the Nazi Constituency*, 37–63.

merely helped confine the terms of bourgeois political discourse within a political language that was inimical not only to Marxism but to their own liberal *Weltanschauung* as well.⁶⁹

The conservatives, on the other hand, were certainly more comfortable with the language of *Berufsstand*, but they also faced serious difficulties arising from evocations of the corporate state. By aggressively emphasizing the role of estate in its appeals to farmers, artisans, shopkeepers, and civil servants, the DNVP, with its intimate and prominent ties to both heavy industry and big agriculture, raised expectations it was simply not prepared to satisfy. Forced to choose between its electoral constituency and powerful organized interests close to the party, the DNVP time and again chose the latter. As a consequence, the conservative espousal of the corporate state gradually lost credibility among precisely those groups attracted by the language of estate.⁷⁰

THE CENTRALITY OF *Berufsstand* in the social vocabulary of German politics cannot be dismissed, however, as the product of a cynical, manipulative strategy by established elites working through the major parties. Between 1920 and 1928, a plethora of small, alternative parties—special interest, single issue, and regional—were founded in direct opposition to the mainstream liberal and conservative parties, and, in each instance, they too employed these same occupational categories and championed strongly corporatist platforms. Parties such as the Reich Party of the German Mittelstand, the Christian-National Peasants and Rural People's party, the German Peasants' party, and the Bavarian Mittelstand party, to mention only a few, proliferated, especially during the woefully mislabeled "Golden Twenties" (1924–1928), and their vituperative criticism of the "Weimar system," their recommendations for an alternative political and economic order, and the vocabulary in which both were formulated are all suggestive.

Each of these middle-class special-interest parties was virulently anti-Marxist, and yet their greatest scorn was heaped on the large national bourgeois parties for allegedly failing to protect the interests of the *Mittelstand* from the organized left, on the one hand, and for "selling out" to powerful business and industrial interests on the other. Virtually all such parties attacked the Weimar parliamentary system for being dominated by big business and big labor and advocated the end of divisive party politics and the creation of some form of corporatist political and economic order.⁷¹ Weimar's partisan politics was destroying the German people and "ruining

⁶⁹ The quotation is from Wilhelm Kulz, "An alle, die starken und reinen Willens sind," in *Materialien zur demokratischen Politik*, nr. 153, August 23, 1930. See Thomas Childers, "Languages of Liberalism: Liberal Political Discourse in the Weimar Republic," in Konrad Jarausch and Larry E. Jones, eds., *In Search of Liberal Germany: Festschrift for Theodore Hamerow* (New York, 1990), 323–59.

⁷⁰ See Attila Chanady, "The Disintegration of the German National People's Party 1924–1930," *Journal of Modern History*, 39 (1967): 67–91; and Manfred Dörr, "Die Deutschnationale Volkspartei 1925 bis 1928" (Ph.D. dissertation, Marburg, 1964).

⁷¹ For an analysis of these "alternative parties," see Thomas Childers, "Interest and Ideology: Anti-System Politics in the Era of Stabilization," in Gerald D. Feldman, ed., *Die Nachwirkungen der Inflation auf die Deutsche Geschichte* (Munich, 1985), 1–19. Not all parties that adopted such language were opposed to parliamentary government. The Catholic Zentrum, for example, endorsed a vague form of classless *Volksgemeinschaft* and employed occupation-specific appeals, often with powerful corporatist overtones, while remaining a supporter—though an increasingly ambivalent one—of the Weimar Republic. Predictably, the Zentrum's occupational appeals were frequently placed within a confessional context. See, for example, "Mittelstand und Zentrum" (August 13, 1930); "An die katholischen Beamten!" (September 6, 1930); "An das christliche Arbeitervolk" (July 1, 1932), all in the party's

every *Gewerbestand* [commercial estate],” the Christian National Peasants and Rural People’s party typically complained. “What we need,” it also typically concluded, “is representation based on occupational estate [*berufsständische Vertretung*].”⁷²

The Reich Party of the German *Mittelstand*, the largest of these alternative parties, concurred, condemning “the modern state with its destructive domination by parties and bureaucracy” and demanding, well before the Great Depression, “an end to this system.”⁷³ “The entire German people, but especially the indigenous *Mittelstand* in town and countryside,” the party asserted, “is striving for a new form of political representation,” and, like so many of the other small, alternative bourgeois parties, it proposed a “tightly knit organization structured by occupational estate” for the “intellectually and physically productive *Mittelstand*.”⁷⁴ Even the more moderate Christian-Social People’s Service agreed, claiming that the *schaffenden Stände* were suffering “bitter distress” and called for “the creation of a *berufsständige* representation” in which the regions [*Stämme*] and *Berufsstände* would be represented and would have specific competencies alongside the parliament.”⁷⁵ Individually, these small parties were utterly insignificant, but, by 1928, a year before the onset of the depression and the dramatic ascent of the NSDAP, such alternative parties attracted roughly one-third of the middle-class electorate, a proportion that surpassed the two liberal parties combined and virtually equaled the conservative totals.⁷⁶

Although the NSDAP was unable to capitalize on such sentiments until the radicalization of German political life during the depression, Nazi corporatist rhetoric was anything but marginal in middle-class political discourse. Before their electoral breakthrough in 1930, the Nazis were often viewed by many middle-class Germans, especially farmers, as “leftist” or “Bolsheviks in nationalist wrapping.” In the aftermath of the 1928 election, from which the Nazis emerged with less than 3 percent of the vote, the party moved to sharpen its focus on the middle-class electorate by, among other things, affirming its commitment to private property and defining its “socialist” orientation in corporatist terms.⁷⁷ As the *Nationalsozialistische Jahrbuch* of 1929 explained, “We reject all effort at social leveling [*Gleichmacherei*]” attempted by the left. “Social policy must be oriented toward the *Berufsstand*. The idea of occupation will be one of the strongest pillars of the coming National Socialist state, of the Third Reich. We know that our national resurgence [*völkischer Aufstieg*] is bound up with a revitalization of occupational life [*Berufsleben*].”⁷⁸

In championing a corporatist society as an alternative to both Marxism and capitalism, the Nazis possessed a major asset that clearly distinguished them from

Germania. On the Zentrum’s growing ambivalence regarding Weimar democracy, see Moeller, *German Peasants and Agrarian Politics*, 143–55; and Childers, *Nazi Voter*, 260–61.

⁷² “Wahlaufuf der Christlich-Nationalen Bauern und Landvolk-Partei,” in *Reichstags-Handbuch IV. Wahlperiode 1928* (Berlin, 1928), 188–90.

⁷³ See the “Wahlaufuf der Reichspartei des deutschen Mittelstandes,” in *ibid.*, 183–86; and the party’s *Wahlaufuf* for the December 1924 campaign, in *Reichstags-Handbuch III. Wahlperiode 1924* (Berlin, 1925), 157–62. See also Martin Schumacher, *Mittelstandsfrent und Republik. Die Wirtschaftspartei-Reichspartei des deutschen Mittelstandes 1919–1933* (Düsseldorf, 1972).

⁷⁴ “Wahlaufuf des Reichspartei des deutschen Mittelstandes,” *Reichstags-Handbuch V. Wahlperiode 1930* (Berlin, 1930), 183–86.

⁷⁵ See Paul Bausch, “Die politischen Gegenwartsaufgaben des Christlich-Sozialen Volksdienstes,” in *Schriften des CSVD*, nr. 8, 1930, BA, ZgS. 1, 279/9.

⁷⁶ Childers, “Interest and Ideology,” 1–19.

⁷⁷ For the Nazi difficulties with the peasantry in the period before 1929 and charges of “Brown Socialism” made regularly by the bourgeois parties against the Nazis, see Childers, *Nazi Voter*, 150–55.

⁷⁸ Franz Stöhr, “Sozialpolitik-Kulturpolitik,” *Nationalsozialistisches Jahrbuch 1929* (Munich, 1929).

their bourgeois rivals. Unlike the liberals or conservatives, the Nazis had never been saddled with government responsibility and hence had never been forced to make policy decisions that might satisfy one group while disappointing others. Unlike the small, special interest or regional parties, the NSDAP could make a relatively credible claim to stand above any specific region or economic group. As a consequence, the National Socialists were not clearly identified with any particular set of interests or policies. In the crowded field of middle-class politics, the NSDAP alone could speak the language of both transcendent class solidarity and sectarian special interest, employing corporatist language to articulate occupation-specific interests—even contradictory ones—while simultaneously attacking the petty, special-interest orientation of all the bourgeois parties and calling for national unity in a *Volkgemeinschaft* that would vanquish class conflict once and for all.

In this context, it is certainly worth noting that the greatest sources of Nazi support in 1932 were located in those occupational groups whose organizations articulated socially exclusive, corporatist views of their socioeconomic position—self-employed artisans, peasants, shopkeepers, and civil servants. Indeed, it is precisely this pattern of support that has prompted Winkler, Kocka, Wehler, and others associated with the new orthodoxy to emphasize the role of pre-industrial vestiges in explaining the failure of democracy and the rise of National Socialism in Germany. The rise of National Socialism, however, cannot be attributed solely or even for the most part to these “pre-industrial vestiges” or to an “anti-modernist” reaction. It is important to remember that support for the NSDAP was not uniformly developed, even in the middle-class electorate. The Nazi constituency was a highly unstable coalition of shifting social forces that evolved and changed significantly. Support for the party was strongest and most durable in the old middle class, among shopkeepers and self-employed artisans, where such corporatist traditions had deep resonances. In every other group, including civil servants and peasants, significant support for the NSDAP could be mobilized only in periods of severe economic crisis—briefly during the inflation and stabilization crises of 1923–1924 and massively between 1928 and 1933.⁷⁹ Even in the latter period, the National Socialist constituency remained extremely volatile, as Nazi political strategists themselves recognized, and signs of its fragmentation were already evident in November 1932. The social dynamics of Nazi political popularity are understandable only if the hard core of its following, drawn primarily from the old middle class and reflecting long-term socio-political affinities emphasized by the orthodox interpretation, is distinguished from the mass of voters who after 1928 responded to the deepening economic crisis by registering a largely negative protest vote against a system that had produced a seemingly endless stream of economic and political disasters.⁸⁰

Moreover, after the revisionist scholarship of the past decade, it is no longer acceptable simply to describe, as the orthodox authors tend to do, an entire social or occupational group as “a pre-industrial stratum with a pre-capitalist economic mentality.”⁸¹ Social reality, as the researches of Blackbourn, Caplan, Moeller, and

⁷⁹ See Childers, *Nazi Voter*, 262–69.

⁸⁰ Thomas Childers, “The Limits of National Socialist Mobilization: The Elections of 6 November 1932 and the Fragmentation of the Nazi Constituency,” in Childers, *Formation of the Nazi Constituency*, 232–59.

⁸¹ The following statement by Winkler is an extreme formulation of the orthodox view: “The commercial *Mittelstand* did not, however, constitute the only pre-industrial element in the National Socialist mass support. Peasants were no less a pre-industrial stratum with a pre-capitalist economic mentality . . . This also applies even to the white-collar employees, who, as Jürgen Kocka has shown,

others have amply demonstrated, was a good deal more complex. On the other hand, the revisionists seem unable to explain how, for example, civil servants or white-collar employees could adopt a "modern" lifestyle—go to the cinema, listen to the radio, and drive an automobile—and yet, in the political arena and elsewhere, express their interests in the corporatist language of estate. Such apparent paradoxes were commonplace in the political culture of the Weimar Republic, and neither claims of manipulation from above nor formulaic invocations of "false consciousness" suffice to explain them.

Perhaps, rather than ascribing a single "mentality" to complex socio-occupational or demographic groups, it would be more fruitful, as Lynn Hunt and George Sheridan have suggested in another context,⁸² to pursue the proposition that individuals operate in their public lives with "multiple identities." After all, an artisan of the "old *Mittelstand*" might also be a veteran, a Protestant, a homeowner, and belong to a wide variety of professional and social clubs or associations, each reflecting one or more sides of a multifaceted public self.⁸³ Certainly, each of the Weimar parties, from the Communists to the National Socialists, sought to engage as broad a range of those multiple identities as possible. They therefore formulated appeals to gender, generation, religion, region, patriotism, class solidarity, and the *Volk*. *Berufsstand* was but one of these social identities, one possible avenue of political appeal. Yet the pervasive presence of *berufsständisch*, corporatist terminology in the political discourse of bourgeois politics throughout the Weimar period is particularly striking. It may have been, as party strategists clearly believed, the most reliable indicator of social self-image in German political culture. In this sense, it hardly matters whether the terminology conformed to social reality or not. Although a *Bauernstand*, much less a *Mittelstand*, may not have existed as a corporative entity in either Wilhelmine or Weimar Germany, when farmers, artisans, shopkeepers, and civil servants defined themselves for political action, they regularly chose to adopt occupational definitions of their social identity and to support political movements that spoke the language of *Berufsstand*. In fact, the appeal of corporatist imagery, with its evocation of stable social status, may have been greatest when the gap between the stability it implied and the grim economic realities of the Weimar era was most pronounced.

HAVING ESTABLISHED THE PROMINENCE OF occupational forms of social address in Weimar political culture, it remains to question whether a description of them as "pre-industrial," "pre-capitalist," or "pre-bourgeois" is either adequate or accurate. Although the terms of socio-political address clearly were rooted in the language of *Stand*, the content that filled those forms had undergone a significant transformation since at least the 1890s and an even more dramatic one since the genuinely

... were also oriented toward pre-industrial *ständisch* traditions"; Winkler, "Die 'neue Linke' und der Faschismus," 79.

⁸² Lynn Hunt and George Sheridan, "Corporatism, Association, and the Language of Labor in France, 1750–1850," *Journal of Modern History*, 58 (December 1986): 831.

⁸³ Rudy Koshar's examination of *Vereinswesen* (associational life) in Marburg offers a useful strategy for analyzing multiple public identities. See his *Social Life, Local Politics, and Nazism*; and his "Contentious Citadel: Bourgeois Crisis and Nazism in Marburg/Lahn, 1880–1933," in Childers, *Formation of the Nazi Constituency*, 11–36.

pre-industrial decades of the early nineteenth century.⁸⁴ In its original, pre-industrial form, the estate provided individual artisans or peasants with a normative conceptualization of who they were and what they did. It defined their legal rights and privileges within society, conferred status, and set certain spiritual and social obligations to the community as well. There were undoubtedly some residues of these normative values in the Weimar Republic, especially among artisans and civil servants,⁸⁵ but, under the wrenching impact of war, revolution, hyperinflation, harsh stabilization, and finally the Great Depression, the terminology of *Berufsstand* had degenerated into a largely defensive orientation that lacked much of its earlier positive content.⁸⁶ The corporatist language found in the appeals and programs of the parties and interest groups that employed it during the Weimar period tended to consist of little more than a bitter complex of negatives that found political expression in an increasingly frantic anti-Marxism, antiliberalism, and ultimately anti-parliamentarianism.⁸⁷

Examining the problem from a different angle, Konrad Jarausch, in his recent work on professionalization, has noted that the use of this terminology actually expanded after roughly 1890, as organizations representing occupational groups with no corporatist past at all, such as white-collar employees and school teachers, adopted a neo-corporatist vocabulary to define their clientele (the *Lehrerstand*). This tendency, Jarausch suggests, should perhaps be viewed less as a symptom of lingering pre-industrial residues than as what might be termed a "post-liberal phenomenon." The proliferation of corporatist terminology in the late nineteenth and early twentieth centuries might, therefore, be interpreted as evidence of increased articulation of special interests in an advanced and highly competitive industrial capitalist economy.⁸⁸ This is, in many ways, a persuasive argument. Yet what makes the German case potentially significant—that is to say, different—is the ease with which, in such efforts at "modern" interest articulation, occupational

⁸⁴ There is no systematic examination of the changing terminology of German political discourse, but the brief description of the evolution of the term *Bürgertum* in James J. Sheehan, *German Liberalism in the Nineteenth Century* (Chicago, 1978), 19–34, 239–57, is a useful point of departure. See also Brunner, Conze, and Koselleck, *Geschichtliche Grundbegriffe*; Jürgen Kocka, "Bürgertum und Bürgerlichkeit als Probleme der deutschen Geschichte vom späten 18. zum frühen 20. Jahrhundert," in his *Bürger und Bürgerlichkeit im 19. Jahrhundert: Deutschland im europäischen Vergleich* (Göttingen, 1987), 21–64.

⁸⁵ For the importance of such norms among artisans in Hamburg during the Weimar Republic, see Frank Domurad, "The Politics of Corporatism: Hamburg Handicrafts in the Late Weimar Republic, 1927–1933," in Richard Bessel and E. K. Feuchtwanger, eds., *Social Change and Political Development in the Weimar Republic* (London, 1982), 174–206.

⁸⁶ Winkler's own analysis of artisans and shopkeepers bears out this largely negative orientation. See Winkler, *Mittelstand, Demokratie, Nationalsozialismus*, 117–20, 157–82.

⁸⁷ In an insightful reexamination of the utility of modernization theory as applied to Weimar, Gerald Feldman correctly notes that the *Mittelstand's* corporatist and social protection program of the 1930s was hardly a call for a return to a pre-industrial past but rather a "charge against the modernity of the present that addressed itself to very real twentieth century circumstances"; Gerald D. Feldman, "The Weimar Republic: A Problem of Modernization?" *Archiv für Sozialgeschichte*, 26 (1986): 5. In other words, one might legitimately speak of "anti-modernist" attitudes within the *Mittelstand* without accepting "the mechanistic characteristics and teleological pretensions which once typified [modernization theory]"; Detlev Peukert has also recently called for a reexamination of our notions of "the modern" and especially for decoupling the conceptualization of "modernization" and "anti-modernism" from modernization theory; Peukert, *Die Weimarer Republik. Krisenjahre der klassischen Moderne* (Frankfurt, 1987), esp. 87–89.

⁸⁸ Konrad Jarausch, *The Unfree Profession: German Lawyers, Teachers, and Engineers, 1900–1950* (New York, 1990). Jarausch has already made a number of important contributions to the study of professionalization in Germany. The most pertinent to the issues are "The Crisis of the German Professions 1918–1933," *Journal of Contemporary History*, 20 (July 1985): 379–98; and "Die Not der geistigen Arbeiter: Oberlehrer, Journalisten und Schriftsteller in der Krise," in J. Thuneke, ed., *Literarischer Nationalsozialismus* (Düsseldorf, 1985).

groups could draw on a powerful corporatist idiom that had remained in the mainstream of German social and political life.

These problems cry out for comparative analysis, with an examination of social structures, modes of political mobilization, and the social vocabulary of political discourse both across national frontiers and over time. Although comparative analysis is clearly beyond the scope of this essay,⁸⁹ some preliminary conclusions on the Weimar case can be drawn. The linguistic forms of *Stand* and *Berufsstand* had outlived the social and economic conditions they had evolved to describe, and their content had become radically transformed, but they nonetheless continued to provide a powerful idiom for German middle-class politics. That the widespread use of such corporatist terminology served, in objective terms, to mask genuine class interests and conflicts is undeniable, but the subjective function of the language should not be minimized. The forms of socio-political address, of social and political self-description, must be taken seriously in their own right. After all, this vocabulary provided middle-class political discourse with its distinctive shape and defined the terms in which politics and society were understood. Although other, more immediate economic and political factors certainly had a more direct and profound impact on the rise of National Socialism, the ability of the NSDAP to mobilize its remarkably heterogeneous following must also be seen, to use Gareth Stedman Jones's phrase, "as a function of its capacity to persuade its constituency to interpret their distress or discontent within the terms of its political language."⁹⁰ Moreover, by confining middle-class political discourse within a linguistic framework antithetical to both Marxism and liberalism, this corporatist vocabulary compelled the parties of the left and the liberal center to operate on a linguistic terrain that was far more congenial to conservative or even fascist politics than their own.

Thus the centrality of occupation in the social vocabulary of Weimar political discourse, with the connotations of *Stand* associated with it, strongly suggests that pre-industrial vestiges did play an important if oblique role in the political culture of the Weimar Republic. Those vestiges, however, are not to be found in the existence of "pre-industrial groups with pre-capitalist economic mentalities" or in manipulative "pre-modern" elites but rather in the very language used by middle-class Germans to define themselves socially for political action. That language, reinforced by social convention, economic organization, and official government usage, may well have operated as an independent cultural variable in German political life, whether or not its users fully appreciated its historical origins or ideological implications. While the meanings of these corporatist terms had been hollowed out and refilled with new content, the words themselves remained at the vortex of political discourse in the Weimar Republic and appeared to have possessed a remarkable evocative power.

The language of *Berufsstand* may not have reflected "objective" social reality, but it appears to have played a major role in structuring social and political perceptions. Thus, if we are to understand the formation of political consciousness and the dynamics of political mobilization in Weimar or elsewhere, the internal structures, idioms, and shifting usages of that language must be systematically examined. We must be prepared, as Roger Chartier has argued, to approach and evaluate

⁸⁹ A large-scale comparative project on the social language of political discourse in England, France, Italy, and Germany from 1870 to 1970 is underway at the Center for West European Studies, University of Pennsylvania.

⁹⁰ Gareth Stedman Jones, "Rethinking Chartism," in *Languages of Class*, 96.

“representations of the social world” as “constituents of social reality.”⁹¹ Yet, in analyzing those representations or languages, we must not lose sight of context or dispense with the issue of agency. Who said it, to whom, and in what context is important. The purpose of such analysis is not to replace a social interpretation with a linguistic one, to substitute the semiological for the social, but to determine how the two relate. It is the interaction of this corporatist language, its forms and constructions deeply embedded in the social vocabulary of German politics, with the economic and political developments of the era from 1871 to 1933 that holds the key to understanding the role of pre-industrial factors in the political culture of modern Germany. But this is only one issue on which the analysis of language might shed new light. On a far more fundamental level, language—the terms and linguistic constructions used by individuals, interest organizations, and political parties—forms a critical interpretive link between social identity and the process of political mobilization, and it is time more historians turned their attention to it.

⁹¹ Roger Chartier, “Intellectual History or Sociocultural History? The French Trajectories,” in Dominick LaCapra and Steven L. Kaplan, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 30.

Modernism and Fascism: The Politics of Culture in Italy, 1903–1922

WALTER L. ADAMSON

But why is the unity of the *Patria* epitomized by the symbol and language of Rome? . . . If Mazzini, if Garibaldi tried three times to enter Rome, and if Garibaldi gave to his red shirts the tragic, inescapable dilemma, “either Rome or death,” then this means that, for the men of the Italian Risorgimento, Rome had an essential function of the first order to fulfill in the new history of the Italian nation. Thus let us too, pure in spirit and without rancor, raise our thoughts to Rome, one of the world’s few cities of the spirit, because at Rome, among those seven history-laden hills, occurred one of the great spiritual wonders that history records. There an Eastern religion, foreign to us, was transformed into a universal religion that recovered in new form that empire that the consular legions of Rome had driven to the ends of the earth. Now we aspire to make of Rome the city of our spirit, a city purged, cleansed of all the elements that have corrupted and violated her; we aspire to make of Rome the pulsating heart, the active spirit of the imperial Italy of our dreams [prolonged applause].

SO SPOKE BENITO MUSSOLINI to the citizens of Udine a month before the March on Rome.¹ Like the March itself, which owed its success not to military force but to its force as mythic theatre, the rhetoric compresses the essential themes of fascist culture into a mythic image.² Rome symbolizes genuine Italian unity, not the mechanical aggregation of post-Risorgimento liberalism but the spiritual unity that had inspired the leaders of the independence movement, Giuseppe Mazzini and Giuseppe Garibaldi. Rome symbolizes, then, a “second Italy,” a “new” Italy, a hitherto-unattained goal. It also symbolizes a uniquely spiritual city through which a universalized Christianity had in effect restored the universality of the Roman empire and, it is implied, through which a new fascist imperium can re-spiritualize the political world.

The one element missing from Mussolini’s vision of Rome is an indication of how he intends to achieve this re-spiritualization. But we learn soon enough. “Violence,” he proclaims a few paragraphs later, “is not immoral.” Insofar as it “heals a cancerous situation, it is highly moral, sacrosanct, and necessary.”³ Myth, a secular-religious quest, the cultural rebirth of a new “spiritual” Italy, regenerative “sacrosanct” violence—these are the essential elements of Mussolinian rhetoric on

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¹ Cited in Antonino Répaci, *La Marcia su Roma* (Milan, 1972), 689.

² For a good statement of this by now well-accepted view of the March on Rome, see Adrian Lyttleton, *The Seizure of Power: Fascism in Italy 1919–1929* (New York, 1973), 85–93.

³ Répaci, *La Marcia su Roma*, 690.

the eve of the March on Rome, and they remain central for him through the final days of his regime. My concern in the present essay, however, is not the development of this rhetoric but rather one of its central sources in the political culture of post-Risorgimento, especially "Giolittian," Italy: the avant-garde "modernist" movement.⁴

Provisionally, "modernism" may be understood in terms of what I take to be the central project of the intellectual generation entering the European cultural scene between 1900 and 1914: that of a "cultural regeneration" through the secular-religious quest for "new values." By "avant-garde modernist movement," then, I refer to those organized cultural groups of intellectuals that sought to advance this project of cultural regeneration, most often through journals but also through publicly staged events. In Italy, the most famous modernist group was the Milan-based futurists. But, for understanding the cultural sources of fascism, especially for Mussolinian rhetoric, the modernist group centered in Florence around the journal *La Voce* is more important. Although I cannot fully document my claims here, this essay argues that the *vocianti* were modernist and not simply anti-democratic or anti-regime; that their modernism represented in embryo what became a fascist conception of and attitude toward life; that Mussolini took the essence of his cultural politics from them; and that his fascism might therefore be characterized in important respects as the politicization of Italian modernism.

Before proceeding, let me dispel some potential misunderstandings of this argument. First, I am not suggesting that most intellectuals attracted to fascism were modernists. On the contrary, most such intellectuals were more explicitly political from the beginning, and modernists represented only a small portion of them. Italian fascist intellectuals fell into four somewhat overlapping groups: syndicalists or ex-syndicalists, Nationalists (members of Enrico Corradini's Italian Nationalist Association), idealists, and modernists. The first two groups were generally positivist and materialist, the latter two, anti-positivist and spiritualist. The Nationalists and idealists represented the more conservative and authoritarian side of fascism, the syndicalists and modernists its more "revolutionary" and aggressive side. Within the last group—the modernists—we may distinguish futurists, followers of Gabriele D'Annunzio, and former associates of *La Voce*.⁵

Second, it should not be thought that all the Italian modernists accepted fascism or that, among those who did, their reasoning was the same or their outlook equally enthusiastic. Giuseppe Borgese was a modernist, at least through 1911, and yet was never attracted to fascism.⁶ Giuseppe Prezolini was attracted only in its early stages and soon became deeply ambivalent.⁷ Still, it is undeniable that fascism claimed, by

⁴ In looking back from fascist culture to its source in pre-war Italian modernism, I do not mean to imply that the direction of influence was not also the reverse, that is, that fascism did not also influence the remnants of modernism that were contemporaneous with it. This relation is simply not my concern here.

⁵ Luigi Pirandello is the odd case here. Artistically a modernist—Paolo Chiarini thinks of him as Italy's lone expressionist—he always remained something of a positivist, and in his adherence to fascism his reasoning seems closer to the traditional authoritarianism and positivism of the Nationalists. See Paolo Chiarini, *L'espressionismo* (Florence, 1969), 38–51; and Gian Franco Venè, *Pirandello fascista* (Milan, 1971).

⁶ Giuseppe Borgese, who collaborated on *Leonardo* and edited *Hermes*, was already turning by 1906 toward a more "establishment" journalist's career, first as editor of *Mattino* (Naples), then as its Berlin correspondent, then in 1911–1912 as a writer for *La Stampa* and *Il Corriere della Sera*. See Fernando Mezzetti, *Borgese e il fascismo* (Palermo, 1978); and Renzo De Felice, "G. A. Borgese irregolare della cultura," *Giornale nuovo* (August 24, 1977).

⁷ After the Matteotti Crisis of 1924, Prezolini became openly critical of fascist methods but retained a high regard and friendship for Mussolini; see his *Manifesto dei conservatori* (Milan, 1972), 88.

far, more Italian modernists than did any other political movement. Only the generation that came to maturity during and after the war, the generation of Antonio Gramsci and Piero Gobetti, made a substantial effort to move Italian modernism in an antifascist direction.

Third, and perhaps most important, it should not be thought that fascism, as movement and ideology, was either a united or a coherent whole. As just indicated, fascist intellectuals alone represented a considerable range of political viewpoint. All good studies of fascism as movement and ideology acknowledge the pluralism within it, but one key ingredient in this pluralism is commonly ignored.

AN APPRECIATION OF THE IMPORTANCE OF THE CONNECTION between the Italian modernist and fascist movements might well begin by recognizing that the modernists, a minority within the fascist movement, were disproportionately influential in legitimating it. Fascism's greatest success in gaining intellectual legitimacy was the recruitment of Giovanni Gentile. Its pretension to be making a revolution of the spirit and a new political culture for Italy would have been much less convincing without him.⁸ Yet the ability of the fascists also to recruit modernists such as Filippo Marinetti, Ardengo Soffici, Luigi Pirandello, and Curzio Malaparte was far from unimportant. They were among the best-known signatories to the 1925 "Manifesto of Fascist Intellectuals." All except Malaparte became members of the Italian Academy, where they were later joined by D'Annunzio and Giovanni Papini.⁹

The relation of these modernist intellectuals to fascism has been misunderstood because of an important paradox in the modernists' legitimation of it. Even as Mussolini himself appropriated a modernist understanding of the world, and even as he used some of its members to legitimate his politics, most of the various fascist modernisms of the postwar period—Marinettian futurism, D'Annunzianism, and Malaparte's *selvaggismo* ("savage-ism") and *strapaesismo* ("super-country-ism")—irritated him. When Mussolini wanted cultural legitimation, he turned to the most prestigious Italian intellectual currents like those led by Gentile and Pirandello. Yet fascism did not cast modernist avant-gardism aside. Mussolini rejected only those—like Malaparte—whom he could not transform. The rest learned to play grand old men rather than cutting-edge revolutionaries.

Even more important than modernism's assistance in legitimating fascism was its role in providing fascism a cultural-practical orientation.¹⁰ It may be that Mussolini was, above all, a masterful political pragmatist and that fascism was essentially an opportunistic response to the world war rather than a movement deeply rooted in an ideological tradition. Still, Mussolini needed ideas, and, since he had few original ones himself, he drew on nearly every anti-establishment movement or philosophy

⁸ From the beginning, Gentile was the major intellectual in Mussolini's cabinets. In early 1925, at a point of genuine crisis for the regime, he authored the "Manifesto of Fascist Intellectuals." Later, he led the project for an Italian Encyclopedia and became the leading light in the Italian Academy.

⁹ On the Manifesto, see Emilio R. Papa, *Fascismo e cultura* (Venice-Padua, 1974), 159–91. On the Italian Academy, see Marinella Ferrarotto, *L'Accademia d'Italia* (Naples, 1977).

¹⁰ As Daniela Coli has written, "We must remember that, in contrast with Hitler, who never failed to show his contempt for culture, Mussolini . . . welcomed all cultural entreaties (from Sorelian myths to LeBon's psychology of the crowd to Spengler's 'Caesarism') as useful in terms of expanding and justifying his personal power, since he considered national culture as an instrument for consolidating the internal and external prestige of the new fascist state"; Coli, "Il caso storiografico Giovanni Gentile," *Studi storici*, 27 (April–June 1986): 512.

to which he was exposed: Nationalism, syndicalism, corporatism, but, above all, Gentilian idealism and *La Voce*an modernism.¹¹ From Gentile, he appropriated a theory of the state, which gave his regime the appearance of an ethical foundation. But his debt to *La Voce* lay even deeper, for its writers gave him not only his central ideas but his closest intellectual association.¹² Indeed, it was probably through them that he first came to appreciate Gentile, whose intellectual status in Italy had been immensely elevated by the esteem the *vocianti* held for him. Like them, Mussolini was attracted by Gentile's almost mystical voluntarism. But, like them, he too preferred Georges Sorel's brand of irrationalism on a more visceral level.¹³ For, in this irrationalism, both Mussolini and the *vocianti* found a crucial source for a cultural regeneration that would transcend the sterility of positivist and materialist "culture" by infusing traditional religious language, symbols, and myths into mass-based, secular institutions.

Although drawing on many sources, the early fascist movement contains little in the way of cultural ideas and ideals that cannot be found in the modernism *La Voce* had most fully represented. Fascism's idea of community emphasized an "integral" civilization that would re-link the cultures of the sciences and humanities. Like the *vocianti*, most fascist leaders believed that industrialism and technology should be embraced but that the culture of positivism with which industrialism and technology were presently bound up was sterile and alienating. Modernity needed a secular surrogate for the increasingly moribund cultures of traditional religion, one that could serve as the basis for a stronger form of human community. To regain this community, cultural practices retrieved from preindustrial society needed to be infused into modern industrial and political settings.

In fact, fascism's emphasis on the importance of restoring the cultural authority of myths derived directly from *La Voce*an modernism. The fascists believed that a strong nation needed a foundation in a common secular-religious practice based on shared political symbols, rituals, and language. In an important work, Emilio Gentile has shown that all pre-war Italian cultural groups were in search of a Sorelian "myth of the new state," the central one of which was *La Voce*'s "myth of the second Italy."¹⁴ The point may even be pressed beyond myth in the political sense deriving from Sorel. As Niccolò Zapponi has written, ours is "the century of 'cimetières marins' and desolate lands, of poetic Apollos and lunar Pierrots, of 'demoiselles' of Avignon and anxious muses. In short, the conviction that we have alienated ourselves from reality (past, present, future), that, to salvage what is salvageable, it is necessary to entrust ourselves to myth, to a surrogate for authentic history—this conviction was in culture (and certainly not only in its most retrograde side) before it was in fascism."¹⁵

¹¹ Mussolini was never a member of the Italian Nationalist Association, nor a committed corporatist, nor a syndicalist in the strict sense, that is, a political organizer for the movement. But, as De Felice has argued, Gentile's thought was an "important and authentic part of Mussolinian culture"; Renzo De Felice, *Mussolini il duce: Gli anni del consenso, 1929–1936* (Turin, 1974), 34–38.

¹² Although Emilio Gentile does not treat *La Voce* as modernist, he has similarly argued that it was a "fundamental and decisive experience" for Mussolini; Emilio Gentile, *Il mito dello Stato nuovo dall'antigiolittismo al fascismo* (Bari, 1982), 103–07. And a central argument of the first volume of De Felice's biography, *Mussolini il rivoluzionario, 1883–1920* (Turin, 1965), is that the core of Mussolini's early socialism is to be found "in the encounter of revolutionary Italian socialism with *La Voce*"; 284.

¹³ On Mussolini's relationship with Sorel, see especially Sergio Romano, "Sorel e Mussolini," *Storia contemporanea*, 15 (1984): 123–31.

¹⁴ E. Gentile, *Il mito dello Stato nuovo*, 54–60.

¹⁵ Niccolò Zapponi, review of Emilio Gentile, *Il mito dello Stato nuovo*, in *Storia contemporanea*, 14 (1983): 540.

IN THE IMMEDIATE AFTERMATH OF FASCISM'S FALL, many interpreters were inclined to treat it as a historical "rupture" or "parenthesis."¹⁶ More recent research, however, clearly establishes that fascism was much closer to everyday life than these interpreters allowed.¹⁷ It was not simply imposed nor was it entirely a manipulation of consent; fascism had a culture that undeniably attracted many. Yet this new appreciation has not been without certain accompanying overreactions, particularly in regard to "fascist ideology." Some have tried to argue that fascism was a "body of doctrine" or "system of thought" comparable in coherence to Marxism or liberalism.¹⁸ But such views miss the essential point: fascism was, especially in Mussolini's hands, a kind of "anti-ideology ideology," which celebrated its own incoherence as a virtue, as indicative of its commitment to the priority of spontaneous action and its contempt for intellectualism.¹⁹ Mussolini fully understood that appealing to "faith" rather than rational (interest-based or utilitarian) argument could be a source of strength for fascism as a political culture.

The important issue, therefore, is not the content of fascist ideology but the cultural sources of fascist rhetoric and of the secular-religious aura it sought to project. Some of these sources are straightforward and have long been understood, even if recent studies have allowed us to appreciate them more fully. World War I produced a "culture of the trenches" that fascism sought to perpetuate.²⁰ The poet D'Annunzio creatively reshaped the rhetorical strategies of secular-religious, political performance from the "radiant days" of May 1915 to his Fiume adventure of 1919–1920.²¹ Yet D'Annunzio was only one, albeit a distinguished exemplar, of a much larger avant-garde, and studies of his relation to fascist culture provoke us to ask whether additional sources of fascist rhetoric might be found in a more general connection with early twentieth-century avant-gardism.

Such a connection was clearly recognized by Italian intellectuals of the period. Despite Benedetto Croce's view that fascism was a "parenthesis" in Italian history, he was readily disposed to implicate *fin-de-siècle* "decadentismo" in the rise of fascism.²² A decade earlier, the younger liberal Piero Gobetti had written that the

¹⁶ See, above all, Benedetto Croce, *Scritti e discorsi politici, 1943–1947*, 2 vols. (Bari, 1963), 1: 7–16, 2: 46–50, 361–62. For a historiographical discussion of this and other "classical" interpretations, see Renzo De Felice, *Interpretations of Fascism*, trans. B. H. Everett (Cambridge, Mass., 1977), 14–75.

¹⁷ In this now-vast literature, the following are among exemplary studies for Italy: Emilio Gentile, *Le origini dell'ideologia fascista (1918–25)* (Bari, 1975); Niccolò Zapponi, *I miti e le ideologie: Storia della cultura italiana 1870–1960* (Naples, 1981); Victoria De Grazia, *The Culture of Consent* (New York, 1981); Gabriele Turi, *Il fascismo e il consenso degli intellettuali* (Bologna, 1980); Mario Isnenghi, *L'educazione del italiano: Il fascismo e l'organizzazione della cultura* (Bologna, 1979); Luisa Mangoni, *L'interventismo della cultura* (Bari, 1974); and Emilio R. Papa, *Fascismo e cultura* (Venice, 1974).

¹⁸ For Italy, a major proponent of this view has been A. James Gregor; see his *Young Mussolini and the Intellectual Origins of Fascism* (Berkeley, Calif., 1979); and *The Ideology of Fascism* (New York, 1969). More recently, and even more controversially, the argument has been extended to France in Zeev Sternhell, *Neither Right nor Left: Fascist Ideology in France*, trans. D. Maisel (Berkeley, 1986).

¹⁹ The idea that all the nineteenth-century ideologies were dead had been given wide currency in Italy by some of Croce's articles in the 1911–1912 period, above all in an interview, "La morte del socialismo," that had appeared in *La Voce*; it is now in *Cultura e vita morale* (Bari, 1955), 150–59. The idea subsequently became a staple of avant-garde rhetoric, and it permeates Mussolini's political writings. See, for example, E. Santarelli, ed., *Scritti politici di Benito Mussolini* (Milan, 1979), 136–39, 206–09, 221–23, 270–74. For a good recent study of fascist ideology that is sensitive to this paradox, see Pier Giorgio Zunino, *L'ideologia del fascismo* (Bologna, 1985).

²⁰ See Ardengo Soffici, *I diari della grande guerra* (Florence, 1986); Curzio Malaparte, *Viva Caporetto!* (Milan, 1981); Piero Melograni, *Storia politica della Grande Guerra* (Bari, 1966); and especially Mario Isnenghi, *Giornali di trincea (1915–1918)* (Turin, 1977); and *Il mito della Grande Guerra* (Rome, 1970).

²¹ See George Mosse, "The Poet and the Exercise of Political Power: Gabriele D'Annunzio," in *Masses and Man* (New York, 1980), 87–103; and Michael Ledeen, *The First Duce* (Baltimore, Md., 1977).

²² See Benedetto Croce, *Storia d'Europa nel secolo decimonono* (Bari, 1981), 301.

generation born in the 1880s "was irreparably romantic, confusedly lost in the fray, insufficiently steady to be realistic; fascism was anticipated before the war by this intellectual futurism."²³ Nor was the point lost on important intellectual adherents of fascism. Giovanni Gentile argued in 1924 that fascism's deepest origins lay in the anti-positivist intellectual climate of the fifteen years prior to the war in which he had been a principal player.²⁴

This early perception was often overlooked in subsequent scholarship, both in studies of Italian intellectual and cultural traditions, most notably of *decadentismo*, and in studies of Italian political movements and ideologies, most notably fascism.²⁵ Writers on *decadentismo* rarely connect pre-war culture to fascism, and writers on fascism seldom look back to pre-war *decadentismo*.²⁶ Moreover, in all too many studies of intellectuals and fascism, historians focus on the "appeal" of fascism to intellectuals, as if fascism were something constituted independently of their activity that they then approached as a finished object to be accepted or rejected.²⁷ Finally, even the early literature tends to overplay the importance of futurism as the source of fascism in the pre-war avant-garde, a bias only now being corrected.²⁸

A reconsideration of Croce's discussion of a connection between *decadentismo* and fascism reveals that he and the literary-critical tradition he inspired in Italy use the word "decadence" in a much broader sense than literary historians have generally employed.²⁹ Rather than referring specifically to the 1880–1900 period, they use *decadentismo* to cover the entire Italian literary and artistic scene from 1880 to 1915. As one recent non-Italian commentator has remarked, it seems to be "a near-perfect synonym for our modernism."³⁰ Therefore, to pursue the Crocean inquiry

²³ Cited in Mangoni, *L'interventismo della cultura*, 62.

²⁴ See Giovanni Gentile, *Il fascismo al governo della scuola* (Palermo, 1924), 307–27.

²⁵ On *decadentismo*, see Walter Binni, *La poetica del decadentismo* (Florence, 1977); Mario Praz, *The Romantic Agony*, trans. A. Davidson (London, 1933); Carlo Salinari, *Miti e coscienza del decadentismo italiano* (Milan, 1973); and Adriano Seroni, *Il decadentismo* (Palermo, 1964). On fascist ideology, in addition to works already cited, see Alexander De Grand, *The Italian Nationalist Association and the Rise of Fascism in Italy* (Lincoln, Neb., 1978); and David D. Roberts, *The Syndicalist Tradition and Italian Fascism* (Chapel Hill, N.C., 1979).

²⁶ An exception is Emilio Gentile; see his *Il mito dello Stato nuovo*; and "From the Cultural Revolt of the Giolittian Era to the Ideology of Fascism," in Frank J. Coppa, ed., *Studies in Modern Italian History* (New York, 1986). Yet even Gentile does not sufficiently recognize the avant-garde, specifically modernist, nature of many non-futurist currents in the pre-war intelligentsia. Before Gentile, the historians who asserted the connection were generally those whose purview was twentieth-century Italian intellectual life as a whole. See, for example, Eugenio Garin, *Cronache di filosofia italiana (1900–1943)*, 2 vols. (Bari, 1955), 1: 26; and Alberto Asor Rosa, *Storia d'Italia: Dall'Unità a oggi* (Turin, 1975), vol. 4, part 2, 1376–77, 1386.

²⁷ The major exemplar of this approach is Alastair Hamilton, *The Appeal of Fascism: A Study of Intellectuals and Fascism, 1919–1945* (New York, 1971). A more recent one is Michel Ostenc, *Intellectuels italiens et fascisme (1915–1929)* (Paris, 1983). Let me, however, hasten to add that there remains much valuable information in both works.

²⁸ This is particularly true of Croce, to a lesser extent of Gobetti. George Mosse helps to counteract the idea in his "Fascism and the Avant-garde," in *Masses and Man*, 229–45.

²⁹ It was precisely in the context of a rising fascism that two of Croce's students wrote the historical studies that launched *decadentismo* as a literary, interpretative concept in Italy. They were Francesco Flora, *Dal romanticismo al futurismo* (Milan, 1921); and Luigi Russo, "Introduzione storica alla prima edizione del 1922–23," in *I narratori* (Milan, 1950), 13–26.

³⁰ Matei Calinescu, *Faces of Modernity: Avant-Garde, Decadence, Kitsch* (Bloomington, Ind., 1977), 219. Calinescu is correct if modernism is defined in the wide sense that includes the generation born in the 1860s as well as the 1880s. My preference is to distinguish the aestheticist or decadent artistic styles of the first generation from the more engaged project of cultural regeneration in the second, and to restrict the term modernism to the latter. More recent Italian studies of *decadentismo* seem to be moving in this direction, that is, using the term more narrowly for the Italian case in the 1880–1903 period, thereby rejecting this aspect of the Crocean heritage. See, for example, Romano Lupérini, *Il Novecento*, 2 vols. (Turin, 1981).

into Italian cultural politics, we need to bear in mind the wider critical literature on European modernism.

In this literature, modernism has been studied in two primary ways. One approach focuses on its socio-historical role, treating it, for example, as an "adversary culture" or "other modernity" that challenges the "modernizing" forces of science and industry, usually in the name of some more "spiritual" alternative.³¹ While modernist movements certainly were adversary cultures, this approach, unless further specified, threatens to leave us with an impossibly large and diffuse object of study. The other approach is more formalist. In discussions of the British literary tradition, for example, the modernist "sensibility" is identified with its accompanying stylistic elements and techniques, such as the importance of the "image."³² This approach helps to sharpen the textual meaning of the concept but all too often retreats to a literary and artistic vacuum in which the social function and meaning of intellectuals and their work become lost. The great virtue of Stephen Spender's discussion—and the reason why I am led to it as a main source of guidance for the Italian case—is that it fruitfully combines both strategies.³³

Briefly, Spender treats modernism as a "sensibility of style and form" that develops because of the "unprecedented modern situation" in which "life-memory" is threatened with destruction.³⁴ The modernists, he suggests, were an "international inter-arts alliance" aiming at "the transformation of the whole of civilization within a revolutionary vision inspired by art." To restore life-memory, they turned to the past, but, because they sought revolutionary transformation, their approach was never one of simple retrieval but rather of sifting the past for remnants of primal—especially mythic—elements of culture.³⁵ Modernists were reacting against the increasing social prominence of scientific and abstract thinking, which threatened to cut them off from a past in which the poetic was intimately and immediately in touch with the sacramental, personal, and natural forces that together made up a ritual of living. Like the romantics of eighty years earlier, the modernists were repulsed by the idea that the imagination should be placed in the service of a rationalist or utilitarian view of life, yet for them there was no hope of a simple return to "nature." Rather, they exalted artifice and the artistic imagination in hopes of shaping a new secular-religious framework that integrated elements from religious tradition, poetic tradition, and contemporary philosophy and art. In short, they intended to create a "new art" that would, in turn, establish a "pattern of hope" and the "symbolism of a shared life." The world would be "pacified and ennobled" in a "revolution of the word."³⁶

As a trans-European description, Spender's ideal type captures extremely well the social and artistic outlook of the generation of intellectuals who came to maturity between 1900 and 1914, and, to a lesser extent, the postwar generation. Moreover, Spender's astute distinction between modernists and their "contemporaries" (intellectuals of the same generation who did not reject the cultural

³¹ See Lionel Trilling, *Beyond Culture* (London, 1966), esp. xi–xviii; and Calinescu, *Faces of Modernity*, esp. 4–5.

³² For two recent examples of research of this type, both of which have much to commend them as scholarship, see Sanford Schwartz, *The Matrix of Modernism* (Princeton, N.J., 1985); and James Longenbach, *Modernist Poetics of History: Pound, Eliot, and the Sense of the Past* (Princeton, 1987).

³³ Stephen Spender, *The Struggle of the Modern* (London, 1963). For another classical and in many ways complementary view of modernism, see Herman Broch, *Hugo von Hofmannsthal and His Time*, trans. M. P. Steinberg (Chicago, 1984).

³⁴ Spender, *Struggle*, 71, 60.

³⁵ Spender, *Struggle*, 259, 209.

³⁶ Spender, *Struggle*, 78, 106, 37–38, 144–46, 257, 83–92.

concomitants of modernization) allows him to acknowledge that not everyone in this generation was modernist and yet to continue to speak of a collective community. Still, Spender's account is weak in that he pays too little attention to the changes in generational outlooks from 1890 onward and, thus, to the historical unfolding of modernism. I will return to this point in the discussion of Italian modernism below.

Several excellent recent studies of the various European modernisms have built on Spender's approach.³⁷ And his approach also converges with recent studies of Friedrich Nietzsche that find the origins of modernism in his peculiar blend of despair and hope, of the need for the artist to face the death of God with the creation of "new values."³⁸ No doubt so protean and multifaceted a concept as modernism will (and probably should) remain contested, but I believe there are persuasive reasons to use it as Spender did; hence my provisional definition of modernism as a project of "cultural regeneration" through the secular-religious quest for "new values."

The practical strategy of the modernists depended first on their perception that civilization had "covered over" what is most basic to human existence—thus the importance of returning to the mythic, legendary, or "primal." While the word civilization here refers to what might now be thought of as the forces of modernization, the modernists opposed not exactly modernization per se but the "positivism" that was, roughly speaking, its cultural concomitant. Unlike many romantics, the modernists rarely thought in terms of a simple return to the past. Rather, they sought a kind of Vichian *ricorso*, or Nietzschean transvaluation of values, or Sorelian heroic regeneration, in which technological, industrial, and (sometimes) national power would be made to coexist with "spiritual" values.

Second, the modernist strategy depended on a certain view of the social role of art. While modernist views of art ran the gamut from expressionism to idealist moralism, common to them all was a stress on the importance of "creativity," one that rejected or at least strongly questioned any view of art as mimesis, a stress on the central role of art in the project of cultural regeneration, and a resolute denial of any normative boundary between "the arts" and society.³⁹

Third, this strategy also stipulated a central role for intellectuals in the creation and organization of a regenerated culture. Modernists experienced the new world of science and industry as a cultural void, believed that some solid cultural counterpart to it needed to be created, and gave themselves the task. Not infrequently, this took the form of an attempt to forge the intellectuals into a kind of "great party," organized around a journal, which published party debates and manifestoes for the edification of the masses.⁴⁰ In this sense, modernists tended toward avant-gardism. It might even be said that modernism recast avant-gardism

³⁷ See, for example, Alan Wilde, *Horizons of Assent* (Baltimore, Md., 1981), 19–49; Peter Jelavich, *Munich and Theatrical Modernism: Politics, Playwriting, and Performance, 1890–1914* (Cambridge, Mass., 1985), esp. 219–22; and Mary Gluck, *Georg Lukács and His Generation, 1900–1918* (Cambridge, Mass., 1985).

³⁸ See especially Allan Megill, *Prophets of Extremity: Nietzsche, Heidegger, Foucault, Derrida* (Berkeley, Calif., 1985), 29–102; Robert Gooding-Williams, "Nietzsche's Pursuit of Modernism," *New German Critique*, 41 (Spring–Summer 1987): 95–108; and Robert Pippin, "Nietzsche and the Origin of the Idea of Modernism," *Inquiry*, 26 (1983): 151–80.

³⁹ The last is a crucial point that can easily be missed when modernism is narrowly discussed as a movement in "the arts." For it was precisely the compartmentalization of "art" that the modernists put at issue. For a recent, and generally good discussion, which nonetheless moves too much in this direction, see Eric Hobsbawm, *The Age of Empire, 1875–1914* (New York, 1987), 219–42.

⁴⁰ For a description of this process in modernist Florence, see Zapponi, *I miti*, 72–87.

during the pre-war period by suggesting that an avant-garde can be strictly cultural, that is, need have no political aim or organization other than its self-appointed and autonomous role as the "party of intellectuals." During and after the war, modernist avant-gardism did become politicized, moving away from the assumption that art and writing by themselves have the power to change human beings and social life. But that complex development lies outside the bounds of the present story, except in the special sense that Italian fascist culture might be seen as one of its instances.

IN APPROACHING ITALIAN MODERNISM, it is essential to bear in mind the socio-economic and political situation of late nineteenth-century Italy.⁴¹ Unified only in 1861, and more by diplomatic maneuvering than mass struggle, the new nation-state faced humbling problems everywhere it turned: politically, in its long history of territorial division and subjugation, the absence of political elites experienced in self-government, and its lack of an established civic culture; socially, in high levels of illiteracy, low levels of technical knowledge and skill, and divisive regional antagonisms; economically, in an antiquated agricultural system, rampant mismanagement in agriculture and railroads, a pronounced gap in development between north and south, paltry natural resources, and tremendous debt (with the attendant threat of economic collapse). Thus, as Italy moved from the ecstatic expectations of the Risorgimento to the sober realities of independence, many experienced the change as an "age of poetry" giving way to an "age of prose."⁴² This sense of the transition was heightened by the decidedly unheroic manner in which Rome was annexed in 1870, a symbolic moment that became a virtual obsession for spiritual-nationalist writers of the next several generations.⁴³

To this new reality, the reaction of cultural intellectuals was harsh, if somewhat contradictory. Certainly, there was widespread recognition of the acuity of former Piedmontese prime minister Massimo D'Azeglio's comment that, Italy having been made, the problem was to "make Italians."⁴⁴ Yet, with few exceptions, the intellectuals turned not to creative political action but to nostalgia for the former "heroic age" mixed with privatistic retreat. "The poet [today]," wrote Giosuè Carducci in 1871, "expresses himself and his moral and artistic convictions as sincerely, honestly, and resolutely as he can; the rest is not his affair."⁴⁵ The cultural intellectuals quickly came to regard the new era as pervaded by pettiness and materialism, hence by a crisis of moral and spiritual values. "There is progress

⁴¹ Some important studies are Benedetto Croce, *Storia d'Italia dal 1871 al 1915* (Bari, 1953); Alberto Aquarone, *Alla ricerca dell'Italia liberale* (Naples, 1972); Edward R. Tannenbaum and Emiliana P. Noether, eds., *Modern Italy: A Topical History since 1861* (New York, 1974); and John Thayer, *Italy and the Great War: Politics and Culture, 1870-1914* (Madison, Wis., 1964).

⁴² These are the famous words of Vittorio Emanuele II, king of Piedmont and then of Italy until 1878, cited in Croce, *Storia*, 2. They also recall Vico's view of history and anticipate the modernist longing for a Vichian ricorso.

⁴³ Typical among them was Alfredo Oriani; see his *La lotta politica in Italia: Origini della lotta attuale (476-1887)* (Milan, 1895). See also the poetry of Giosuè Carducci, for example, his *Giambi ed Epodi* [1878], ed. E. Palmieri (Bologna, 1959). His disillusionment is also well expressed in a letter of 1878 to Arcangelo Ghisleri now in *La scapigliatura democratica: Carteggi di Arcangelo Ghisleri, 1875-1890*, ed. P. C. Masini (Milan, 1961), 139.

⁴⁴ Cited in Christopher Seton-Watson, *Italy from Liberalism to Fascism, 1870-1925* (London, 1967), 13.

⁴⁵ Cited in Asor Rosa, *Storia*, 825.

in science but decadence in life," wrote the critic Francesco De Sanctis.⁴⁶ Parliament and capitalist enterprise epitomized this decadence; the intellectuals experienced themselves as powerless in the face of it.⁴⁷

No doubt part of the intellectuals' frustration lay in the difficulty of forging a coherent political alternative and a new political culture that they could respect. Initially, the so-called historical left (*Sinistra storica*) of the Liberal party generated some enthusiasm, but this evaporated once the left took power in 1876. Intellectuals perceived established politics to be based entirely on a system of *trasformismo*—the practice of coopting the oppositional political elites into the governing majority through various forms of corruption. Not only did such a practice ensure secure economic control for a small elite of northern industrialists but it also (and for intellectuals, more importantly) effectively choked off serious debate on the country's problems. Socialism was weak, given the small industrial work force, and, as it emerged, alternated between the extremes of anarchist utopianism and collaboratist reformism.⁴⁸ Catholic opposition parties were effectively stymied by their "intransigent" aim of separating from the Italian state rather than reforming it. Part of the intellectuals' frustration may also have arisen from the fact that, as a group, they were singularly ill suited to the demands of the new society. Classical and humanist by education, they not only lacked the sort of expertise it would increasingly demand of them but celebrated that lack.⁴⁹ Yet, whatever the sources of their post-Risorgimento disillusion, it offered fertile ground for the development of a potent modernist avant-garde.

This avant-garde emerged from two quite self-conscious generations.⁵⁰ The first is that of D'Annunzio, Croce, Gentile, and Pirandello. Born between 1863 and 1867 (except for the precocious Gentile, who was born in 1875), they entered the cultural scene in the 1890s, when positivism was as powerful as it would ever be in Italy and cultural pessimism among intellectuals as deeply entrenched.⁵¹ Each reacted

⁴⁶ Francesco De Sanctis, "Studio sopra Emilio Zola" [1878], in *L'arte, la scienza e la vita*, ed. M. T. Lanza (Turin, 1972), 413. See also his celebrated essay, "La scienza e la vita" [1872], in the same volume, 316–40. The sense in this passage of being "between two worlds" is a constant theme of Italian modernism and of many of its counterparts elsewhere.

⁴⁷ Thus De Sanctis wrote in 1872, "Today our country exists; the younger generation finds itself between a satisfied ideal and another one to come that is not yet clear; they are without bearings, without a sense of the future, and yet they think of their situation as positive"; *L'arte, la scienza e la vita*, 307–08.

⁴⁸ The Italian Federation of the International Workingmen's Association or First International, established at Rimini in 1872, was strongly anarchist; so, too, was the Revolutionary Socialist party of Romagna (1881) and its nation-wide counterpart created three years later. In 1892, an Italian Socialist party (PSI) emerged in Milan that was completely antagonistic to anarchism. This was the party, under Filippo Turati, that garnered the near-universal deprecation of the modernist intellectuals by "collaborating" with Giolitti's parliamentary majority from 1903 to 1914.

⁴⁹ According to Thayer, school enrollments in science increased only about 50 percent between 1880 and 1890, while classical studies doubled; Thayer, *Italy and the Great War*, 13.

⁵⁰ These generations were noted by Giuseppe Prezzolini, *La cultura italiana* (Milan, 1938), 87–94. They run roughly parallel to the trans-European ones set forth in Robert Wohl, "The Generation of 1914 and Modernism," in Monique Chefdor, et al., eds., *Modernism: Challenges and Perspectives* (Urbana, Ill., 1986), 66–78; the German ones set forth in Henry Pachter, "Expressionism and Cafe Culture," in Stephen Bronner and Douglas Kellner, eds., *Passion and Rebellion: The Expressionist Heritage* (South Hadley, Mass., 1983), 3–39; and the Hungarian ones set forth in Gluck, *Georg Lukács and His Generation*. Interestingly, Gluck also added an intermediate generation born in the 1870s, which, she argued, was more conventionally political than the modernist generation born in the 1880s. In Italy, there is something of a parallel phenomenon with the syndicalists, many of whom—Arturo Labriola (1873), A. O. Olivetti (1874), Paolo Orano (1875), and Enrico Leone (1878)—were of that generation.

⁵¹ My use of the concept of generation primarily reflects clusters of birth dates, but I do make allowances (as in the cases of Gentile and Marinetti) where the cohort group draws heavily on those slightly older or younger. Generations are not mathematical but cultural and inter-subjective categories, and they thereby inevitably carry with them a certain definitional imprecision.

against positivism in a distinctive way, and together their reception of the Nietzschean and Italian idealist (especially Vichian) traditions built the foundation for the modernist theme of cultural regeneration.⁵² But they remained great intellectuals of the traditional sort and made no effort to create avant-garde cultural movements.

This step was taken only by the second generation—Soffici, Papini, Prezolini, and Marinetti.⁵³ Born between 1879 and 1882 (except for Marinetti, who was born in 1876 but wrote little of significance until 1909), this generation built an Italian cultural avant-garde through movements associated with journals, developed opposition to the liberal state into a modernist “party of the intellectuals,” and saw Italian intervention in World War I as the great moment for the heroic regeneration of modern life. Although divided between Milanese futurism and a more moralist and traditionalist (*passatista*) Florentine group clustered primarily around *La Voce*, they shared many enthusiasms and became increasingly interconnected as the war approached.⁵⁴

Like its counterparts elsewhere, Italian modernism developed in response to a crisis of values in European society.⁵⁵ But, in Italy, the response oscillated between two poles—one aestheticist and “decadent,” the other moralist and idealist—with the dominant emphasis on the moralist and idealist. This division is already apparent in the intellectual culture of the 1870s. The exhaustion of the romantic-Risorgimental tradition led both to a bohemian intellectual culture that produced *verismo* (roughly, Italian naturalism) and to a moral-spiritual and proto-nationalistic culture that pined for the civic ethos of the Risorgimento but despaired of reviving it. The major figure in *verismo* was the novelist Giovanni Verga (1840–1922), a Sicilian living the 1880s in the bohemian quarter of Milan. In the spiritual-nationalist culture, the major figure was the poet Carducci (1835–1907), a Tuscan living in Bologna. While it might be said that Verga played Zola to D’Annunzio’s Huysmans, it is also true that D’Annunzio was sometimes quite Carduccian, as in his poetic magnum opus, the *Laudi* (1903). It would be difficult, however, to think of any prominent Italian writer of D’Annunzio’s generation who, centered in the Carduccian tradition, flirted with decadence or bohemia.⁵⁶

Even though D’Annunzio began his literary career as a poet importing French

⁵² On this generation, see Asor Rosa, *Storia*, 821–1098; Luperini, *Il Novecento*, 1: 1–44; Richard Drake, *Byzantium for Rome: The Politics of Nostalgia in Umbertian Italy, 1878–1900* (Chapel Hill, N.C., 1980); Paolo Alatri, *Gabriele D’Annunzio* (Turin, 1983); Emilio Agazzi, *Il giovane Croce e il marxismo* (Turin, 1962); Arcangelo Leone de Castris, *Il decadentismo italiano: Svevo, Pirandello, D’Annunzio* (Bari, 1974); and Luisa Mangoni, *Una crisi fine secolo: La cultura italiana e la Francia fra Otto e Novecento* (Turin, 1985).

⁵³ Other intellectual figures of importance in this generation are Giovanni Amendola (1882), Giuseppe Antonio Borgese (1882), Guido De Ruggiero (1882), and Benito Mussolini (1883).

⁵⁴ On this generation, in addition to works cited elsewhere in these notes, see Asor Rosa, *Storia*, 1099–1357; Romano Luperini, *Gli esordi nel Novecento e l’esperienza della “Voce”* (Rome, 1976); Emilio Gentile, *La Voce e l’età giolittiana* (Milan, 1972); Antonio Santucci, *Il pragmatismo in Italia* (Milan, 1963); and Janvier Lovreglio, *Giovanni Papini*, 2 vols. (Paris, 1973).

⁵⁵ There is also a second generation of modernists born in the decade between 1891 and 1901, who entered the cultural scene on the eve of the war and came into prominence only in the years that followed it. In Italy, prominent members of this generation were Antonio Gramsci, Curzio Malaparte, and Piero Gobetti. For them, the idea of a politically nonaligned modernist counterculture of intellectuals becomes unthinkable, yet they retain enough of the spirit of the previous generation to politicize (rather than simply abandon) modernism, sometimes collaborating together despite differing political ideologies and affiliations. Given the focus of the present essay, their story is left untold.

⁵⁶ Even in the next generation, such deviations were infrequent. The best example would probably be the flirtation of Papini and Soffici with futurism, but it was brief (1913–1915) and never wholehearted.

decadent fashion into the Rome of the 1880s, it was as a novelist that he made his international reputation. From 1889, when his highly acclaimed *Il piacere* appeared, to *Il fuoco*, in 1900, he produced six novels. Each revolves around a male protagonist in search of spiritual meaning, mostly by means of sensual pleasure. Initially, in *Il piacere*, the hero is isolated, self-absorbed, and weak-willed, more romantically involved but otherwise similar to his counterpart in Joris-Karl Huysmans's *A Rebours* (1884). In 1892, however, D'Annunzio discovered Nietzsche through extracts in the Parisian *Revue blanche*, and his aesthetic began to shift toward the celebration of action, violence, misogyny, and the myth of the superior man "beyond good and evil."⁵⁷ Both the novels and the occasional articles of this period make it plain that Nietzsche's ideas of the superman and will-to-power are understood both biologically and as "doctrines."⁵⁸ The result is a myth of "two races"—nobles and plebes—scripted as the story of the noble artist-creator who mesmerizes, violates, and then discards the plebian masses much as he does women.⁵⁹ Although D'Annunzio also flirted with the Wagnerian notion of the artist as the creator of a new secular-religious faith, his stance remained individualist and aloof. His avant-gardism, such as it was, came much later: in the "radiant days" of May 1915 and, after the war, at Fiume.⁶⁰

Thus, while D'Annunzio was widely read by the next generation, he offered them neither the "new faith" nor the model of an avant-garde intellectual for which they were searching, and his reputation among them, initially high, steadily declined over the course of the first decade of the twentieth century.⁶¹ Just the opposite was the case with Benedetto Croce (ironically, in view of the historical judgment he would ultimately make of them).⁶²

By its sharp division of "theoretical" cognition and "practical" volition, Croce's early philosophy incorporated the separation of private and public that his generation had experienced. Because this *distinzionismo* seemed to undercut the possibility of a dramatic cultural movement to transform public life, Croce's philosophy was ultimately dismissed by most of the modernist avant-garde. Yet its initial impact, through the *Estetica* (1902), was quite positive. Indeed, more than any other single work, the *Estetica* was a call to arms for the entire next generation to remake Italian cultural life. Celebrating the independence of art as a human activity, it seemed to suggest that art—and not economy or politics or science—was

⁵⁷ See Guy Tosi, "D'Annunzio découvre Nietzsche," *Italianistica*, 2, 3 (1973): 481–513. The *Revue blanche* article by Jean de Néthy (April 1892) excerpted and summarized Nietzsche mostly on the basis of *The Genealogy of Morals*. It was followed by extracts from *Beyond Good and Evil* (in August), from *Thus Spake Zarathustra* (in November), and from "The Problem of Socrates," *The Twilight of the Idols*, and *Nietzsche contra Wagner* (in 1897). All are now available in a reprint edition from Statkine (Geneva, 1968).

⁵⁸ For the articles, see Alighiero Castelli, ed., *Pagine disperse di Gabriele D'Annunzio* (Rome, 1913), esp. 544–88.

⁵⁹ See especially Stelio's long speech that opens *Il fuoco*.

⁶⁰ For his view of Wagner, see Castelli, ed., *Pagine disperse*, 572–88. Nietzsche's break with Wagner was either unknown to D'Annunzio or ignored by him as an unnecessary complication.

⁶¹ Even Marinetti, who is generally regarded as the most indebted to D'Annunzio among the next generation, treated him as a father figure to be rejected, as in his 1908 polemic *Les Dieux s'en vont, et D'Annunzio reste*. An earlier work, *D'Annunzio intime* (1903), was somewhat more positive. Similarly, Borgese's 1909 book, the fullest critical assessment of D'Annunzio in the pre-war period, was quite negative, especially in its second part, even though he too had been relatively positive in earlier writing. Compare his "L'opera poetica di Gabriele D'Annunzio," *Nuova antologia* (1903), with his *Gabriele D'Annunzio*, 3d edn. (Milan, 1951).

⁶² For Croce's influence on the generation of 1880, see Edmund Jacobitti, *Revolutionary Humanism and Historicism in Modern Italy* (New Haven, Conn., 1981), 57–164. For Croce's ideas and their continued relevance to the discussion of modernity, see David D. Roberts, *Benedetto Croce and the Uses of Historicism* (Berkeley, Calif., 1987).

the paradigmatic creative activity through which the entire world might be transformed. Art was "pure intuition" and, as such, the most fundamental creative act, one on which the "practical" realm, including historical and political knowledge, might be modeled. Drawing heavily on Giambattista Vico's *Scienza nuova* (1725), Croce portrayed poets as "creators," able because of their "poetic wisdom" to work out the concrete forms of social institutions.⁶³ Yet, unlike Vico, Croce did not confine their hegemony to a particular historical era; poetic creativity was a perpetual historical possibility.⁶⁴

Another of Croce's great attractions for the younger generation lay in his assessment of the modern situation. In Croce's view, the great failure of positivism was cultural: "it leaves unsatisfied humanity's religious need." For this reason, "the whole contemporary world is again in search of a religion."⁶⁵ Some, like the Catholic modernists, had sought to reinvigorate traditional religion; others had tried to create a new "religion" out of Marxism; still others were seeking to reanimate the world "via the speculative method of philosophy itself." The last was for Croce the preferred path, even though he preferred his own brand of idealism to the stronger and more irrationalist concoctions that attracted many modernists: pragmatism, "Bergsonianism," and Gentilian "actualism."

Like Croce, Gentile was a source of inspiration for the modernist avant-garde, even though he himself had little use for artistic experimentalism and kept his associations with the modernists as guarded as did Croce. Yet, unlike Croce, he wholeheartedly shared their enthusiasm for "spiritual regeneration"⁶⁶ and was prepared to turn his philosophy into an activist religious faith.⁶⁷ In the post-1911 period especially, his writings greatly attracted the *vocianti* precisely because his idealism came with none of Croce's limitations: it did not split theory and practice but, with its concept of spirit as "pure act," seemed to posit the kind of unlimited historical creativity that Croce's *Estetica* promised but that Croce himself disavowed. In 1912, Prezzolini had tried to refocus *La Voce* as an organ of "militant idealism."⁶⁸ While Soffici and Papini were busy with *Lacerba*, Prezzolini was printing a historic debate between Croce and Gentile.⁶⁹ Indeed, important intellectual contact between Prezzolini and Gentile had begun by 1910. In a revealing exchange of letters concerning the religious education of children, Prezzolini argued that the only non-hypocritical approach would be to use philosophy to give them a new secular religion, while Gentile insisted on the continued viability of traditional religion, at

⁶³ In April 1901, Croce published "Giambattista Vico primo scopritore della scienza estetica," in the Neapolitan journal *Flegrea*. This paper was then incorporated into the first five chapters of the second part of the *Estetica*.

⁶⁴ See Croce, *Estetica*, 9th edn. (Bari, 1950), 256–57.

⁶⁵ Croce, "Per la rinascita dell'idealismo" [1908], now in *Cultura e vita morale*, 35–36. For a fuller discussion of Croce's view of the modern situation, see Walter L. Adamson, "Benedetto Croce and the Death of Ideology," *Journal of Modern History*, 55 (June 1983): 208–36.

⁶⁶ There were differences of emphasis between Gentile and the *vocianti* on this theme. Gentile's treatment of it relied less on his "actualist" metaphysics than on a certain reading of political tradition. The goal of spiritual regeneration for him was an "ethical state"—a concept that harkened back to Mazzini more than to any philosopher. In contrast, the *vocianti* treated the theme—in Nietzsche, James, Bergson, Croce, and ultimately, Gentilean actualism—with little attention to political tradition.

⁶⁷ Croce formulated his very different "religione della libertà" only during the fascist regime after 1924. And, as such, its spirit was more conservative than activist.

⁶⁸ One important indication of the Gentilian turn of *La Voce* is the new collaboration of Guido De Ruggiero in 1912. Other prominent Gentilians involved with the journal are Giuseppe Lombardo-Radice (1879) and Vito Fazio-Allmayer (1885). The phrase "review of militant idealism" became *La Voce*'s subtitle in 1914.

⁶⁹ See the articles by Croce and Gentile, both titled "Intorno all'idealismo attuale," in Prezzolini, *La Voce*, 506–35.

least as the “first moment” in the soul’s upward path. By 1918, in a letter full of traditional religious imagery, Prezzolini exhorted Gentile to lead “the younger generation to believe that they live a *new* life, that they are creating something in the world, that they are a springtime.”⁷⁰

In addition to Gentile and Croce, the modernist generation was deeply influenced by Georges Sorel and, more generally, by that rich, late nineteenth-century tradition of French philosophical moralism—from Ernest Renan to Charles Péguy—in which Sorel played a central role. When De Sanctis in 1872 wrote that “there remains a final program . . . [of] moral and intellectual redemption [which] . . . was not given to Mazzini, nor to the present generation, but to the new one,” his inspiration was Renan’s *La Réforme intellectuelle et morale* (1871).⁷¹ This idea of a “moral and intellectual redemption”—of Italy in De Sanctis, of modern life in Renan—in turn inspired Sorel who, more than any other writer, created the late nineteenth-century religious paradigm in radical cultural politics by combining a global pessimism about the current state of the world with an apocalyptic vision of deliverance. Sorel’s vision had four dimensions (which his readers could draw on singly or in combination): a political-economic one (until 1910, “revolutionary syndicalism”), a spiritual-political one focused on the way “myths” could be generated to galvanize a mass following (as Christianity had done in the early Roman empire), a historical one (which read Marxism as evidence for a Vichian *ricorso*), and a specific commitment to regenerative violence as the catharsis through which the old might be banished and the new ushered in. All four notions had much influence on pre-war modernists as well as on postwar fascists.

But the modernists also needed inspiration for a specific organizational form of cultural practice. Here, they were most influenced by Péguy, whose *Cahiers de la quinzaine*, begun at the end of 1899, became a prototype for Italian avant-garde journals.⁷² Péguy’s journal connected the philosophical search for secular religion with the cultural project of moral and intellectual redemption; it also showed how to communicate through a simple, personal, and lyrically cadenced prose. Even after Péguy’s search ended in a rededication to orthodox Catholicism in 1908, he continued to be influential, and more than one of the Italian modernists saw the logic of his choice when their own project shattered in World War I.

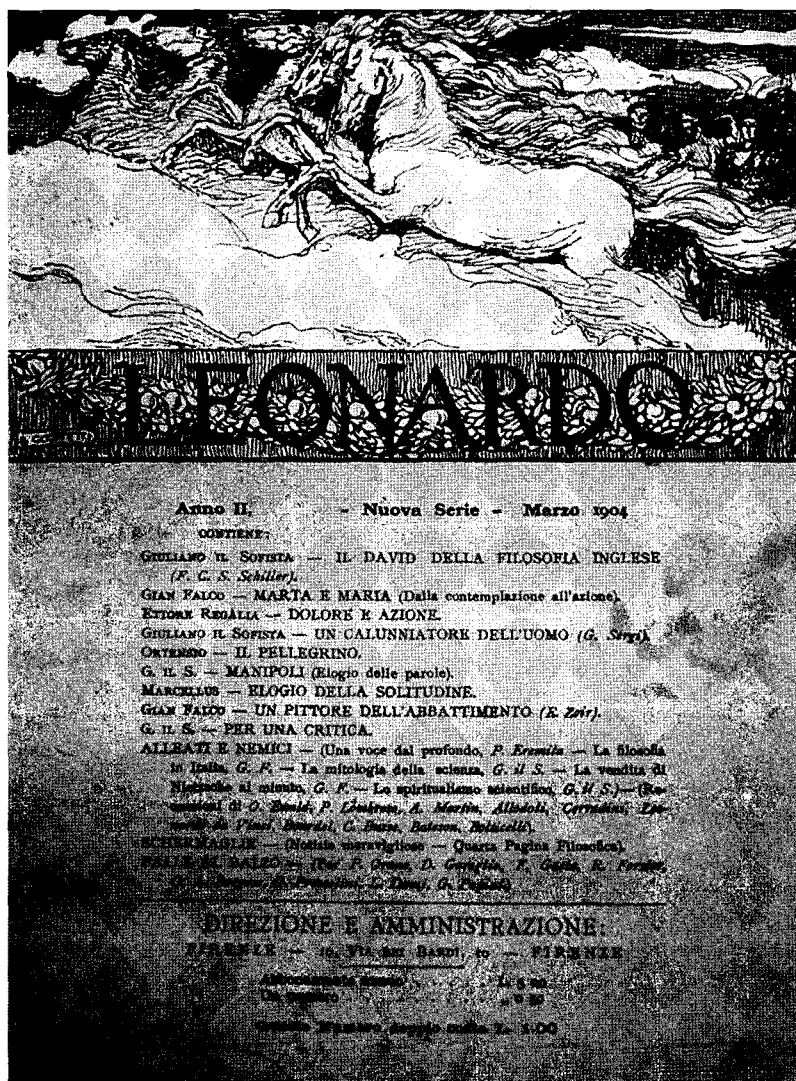
THE YEAR 1903 WAS A WATERSHED in Italian history. D’Annunzio’s pre-war literary career hit its summit with the publication of his *Laudi*; the first volume of Croce’s *La Critica* was published; Italy’s greatest leader since independence and the great political antagonist of the modernist avant-garde—Giovanni Giolitti—returned to power; and this same avant-garde first thrust itself on the cultural scene with the journal *Leonardo*.⁷³ It was a year, then, not so much of transition between

⁷⁰ Emphasis in original. See the discussion in Sergio Romano, *Giovanni Gentile: La filosofia al potere* (Milan, 1984), 114–16, 160–62, and 321.

⁷¹ The sentence became a challenge to the Florentine avant-garde and was frequently cited (albeit without an exact indication of its origin); see, for example, Prezzolini, *La Voce*, 683. For the connection with Renan, see Sergio Landucci, *Cultura e ideologia in Francesco De Sanctis* (Milan, 1977), 406–07.

⁷² See, for example, Prezzolini’s celebration of it in 1910, in Prezzolini, *La Voce*, 564.

⁷³ Giolitti had been prime minister in 1892–1893, but it is his three terms in the first two decades of this century—from November 1903 to March 1905, from May 1906 to December 1909, and from March 1911 to March 1914—that mark the political system known as *giolittismo*, which was associated, especially by his opponents, both with electoral corruption and cooption of the labor movement. He served a last term as prime minister from June 1920 to June 1921.



The cover of the March 1904 issue of *Leonardo* with a drawing by Giovanni Costetti, who had studied with Auguste Rodin in Paris. The issue featured an article by Prezzolini on the British pragmatist philosopher, F. C. S. Schiller.

generations as of the intensification of their cultural combat. The rising generation viewed their two major heritages from the previous generation—D'Annunzian decadentism and Crocean idealism—as varying and perhaps complementary approaches to the same crucial problem: the need for cultural renewal based on secular-religious transformation. But neither of these heritages made clear how intellectuals might actually lead this effort; for relevant examples, they looked northward to Paris, where all the key figures in the Italian avant-garde, including Papini, Prezzolini, Soffici, and Marinetti, had crucial formative experiences.⁷⁴

⁷⁴ And virtually all the others were influenced intellectually by the French. In a study of Borgese in this connection, Giovanni Santangelo described the entire Florentine atmosphere of 1900 as "francesco"; "Borgese e la cultura francese," *Problemi*, 68 (September–December 1983): 273.



A woodcut by Ardengo Soffici, titled "Don Quixote in Tuscany," which appeared as the cover of the August 1906 issue of *Leonardo*. Quite popular with Soffici and his co-workers on *La Voce*, the motif of Don Quixote represented their sense of themselves as lone crusaders for a "new Italy."

Centered in Florence, the avant-garde was relatively unaffected by the emerging industrialism of Milan, Turin, and Genoa, and the new political movements and pressures for mass participation that accompanied it. Not until 1905, with the birth of Marinetti's journal *Poesia*, did the modernist avant-garde make its Milanese debut. Futurism was still six years off. Yet, for the modernists, the impending threat of materialist civilization to Florence was as unmistakable as their own desire to block it. As of 1903, however, they harbored few doubts about their power to

stem the tide, given that mass politics were still new and Giolitti's government had not developed any substantial cultural apparatus of its own.⁷⁵

But what was it, more exactly, that made the Florentine avant-garde "modernist"? First, their cultural engagement had a pronounced generational quality.⁷⁶ *Leonardo* announced its program on the first page of its first issue: "A group of young people [*giovani*], yearning for liberation and universality, eagerly seeking a superior intellectual life, have gathered together in Florence under the auspicious symbolic name *Leonardo* in order to stimulate our existence, elevate our thoughts, celebrate our art."⁷⁷ This generational base was affirmed again and again in subsequent years.⁷⁸ While undoubtedly convinced that it could succeed where two generations of unified Italians had failed, the avant-garde's reassertions of generational unity also helped to compensate for many internal disagreements. For, while the *leonardiani* pictured themselves as sharing "paganism and individualism in life," "personalism and idealism in thought," and a "love of the ideal transfiguration of life in art," there was in fact a tremendous diversity of viewpoint in their midst, as the frequent schisms and quarrels of the next decade amply testify.⁷⁹ They shared antagonisms: to positivism, the academy, economic elites, parliament, and bureaucracies. And they shared a goal: creation of a modern Italian culture. But their conceptions of the new philosophico-religious outlook that would best undergird their new culture were as various as the anti-positivisms then available.

Second, the Florentine avant-garde was modernist because experimental—stylistically as well as in content. It pioneered an elliptical, aphoristic, and fragmentary style, very different from the overblown and ornate forms that, despite D'Annunzio's professed Nietzscheanism, had continued to typify D'Annunzian prose.⁸⁰ Distortion, exaggeration, and telegraphic effects that paralleled modernist development elsewhere especially characterized the successors to *Leonardo*—*La Voce* (1908–1914) and its more futurist offshoot, *Lacerba* (1913–1915).⁸¹ Yet, even in the most experimental work, traces of traditionalism (*passatismo*) and provincialism (the assertion of Italian regional values) remained. To give but one example, Soffici's novel *Lemmonio Boreo*, a celebration of traditional Tuscan life (*toscanità*) often cited as a prototypical literary expression of fascism because of its portrayal of a marauding band of Tuscan nihilists, appeared in 1912—immediately after his book on Arthur Rimbaud (1911) and just before his most radical experimentalism as a writer and artist.⁸²

Third, and above all, the Florentines were modernists because, as Papini wrote to Prezzolini in late 1907, they believed they had "a great mission in the world," that

⁷⁵ For an analysis of the rise of mass journalism and government responses to it, see Valerio Castronovo, *La stampa italiana dall'Unità al fascismo* (Bari, 1973).

⁷⁶ For a fine portrait of them from this angle, see Robert Wohl, *The Generation of 1914* (Cambridge, Mass., 1979), 160–68.

⁷⁷ "Programma sintetico," in Delia Frigessi, ed., *La cultura italiana del '900 attraverso le riviste*, vols. I and II: *Leonardo*, *Hermes*, *Il Regno* (Turin, 1960), 89, emphasis in original.

⁷⁸ For another important statement, see Scipio Slataper, "Ai giovani intelligenti d'Italia" [August 26, 1909], in Prezzolini, *La Voce*, 242–46.

⁷⁹ "Programma sintetico," 89.

⁸⁰ On the "frammentismo" of *La Voce*, see Giuseppe Prezzolini, *L'Italiano inutile* (Florence, 1960), 169–72. A good literary example by a *vociano* is Giovanni Boine's *Il peccato* (1914). For an extended discussion, see Luperini, *Il Novecento*, 1: 181–312.

⁸¹ Technically, *La Voce* continued until the end of 1916, but after the editorial shift in December 1914 from Prezzolini to Giuseppe De Robertis, the journal took on a very different character and no longer played an avant-garde role.

⁸² See, for example, the selections from his *BiF&ZF + 18* (1915) in Luciano De Maria, ed., *Marinetti e il futurismo* (Milan, 1973), 462–72.



A drawing of Prezzolini by Armando Spadini, which appeared in the November 1904 issue of *Leonardo*, and the covers of some of the early Prezzolini's more philosophical books.

of "making the revolution," building "a party of the intellectuals," writing "the New Testament to complement the *Communist Manifesto*," so that "spiritual life" would again "predominate in the world." Indeed, the Florentine modernists were distinguished from their counterparts in Paris, Vienna, or Berlin by the intensity with which they believed in their own right and ability to rule. As they surveyed their political situation, they saw neither a strong liberal tradition nor a nationalist



Giovanni Papini around 1907 and the covers of some of his early books.

one such as developed in Paris and Vienna in the 1890s and in Germany throughout the nineteenth century by way of the tradition of “new politics.”⁸³ Yet they felt the need for such a tradition, especially of a spiritualist sort, and believed it was their own mission to create the Italy in which it could take root. In consequence, their modernism became a unique blending of folk-ish styles and themes (*toscanità*) with cosmopolitan and “high cultural” ones.

⁸³ George Mosse, *The Nationalization of the Masses* (New York, 1975).

"In the *Manifesto*," wrote Papini, "there was land, matter, the economic Messiah; we must give spiritual significance to the Kingdom of God and the Holy Spirit."⁸⁴ Papini sought his new religion in Jamesian pragmatism; Prezzolini in Nietzsche and Henri Bergson, later in Croce, still later in Gentile. Much less the philosopher, Soffici nonetheless despaired in 1904 that "all the gods are dead," and he clearly hoped to find in the French artistic and poetic tradition after Charles Baudelaire a surrogate for traditional religion.⁸⁵ What held the three (and others like them) together was the hope of turning these appropriated cultural-philosophical visions into a new, collectively shared faith, a Sorelian myth that would integrate Italy spiritually and promote the formation of a vigorous new political elite. While devouring Bergson, Prezzolini foresaw "a second Italy."⁸⁶ In his autobiography of 1912, Papini wrote that, although unsure what the "national goal" should be, he knew that it would somehow involve "the preparation of 'spiritual dominion,'" since "Italy had always privileged spirit, and from her might be born a definitive reign of spirit."⁸⁷ Two years later, in a darker mood, Papini passed in review all the philosophical and political ideas that had interested him over the decade, declared them all wanting, and yet prophesied that out of "the universal night of the idols" would come a "new order."⁸⁸

No less modernist was Marinetti's *Poesia*, though, in contrast to the Florentine journals, it offered no program, only a chaotic recycling of French symbolist writings mixed with others derivative of them.⁸⁹ But, in 1909, with the publication of the first futurist manifesto, Marinetti had made a brilliantly original stroke. In his hands, the "manifesto" became a new poetic form and an ideological celebration of the new—in both ways, a demonstration of how art could become mass cultural spectacle. Over the next few years, his ability to enact his manifestoes as a new mode of cultural performance distinguished futurism as the most artistically original expression of Italian modernism.

Like Soffici, Marinetti had been nurtured on the French avant-garde, especially Alfred Jarry and, from the next generation, Guillaume Apollinaire and Blaise Cendrars. To these, he added a taste for Bergson, whose idea that consciousness is an uninterrupted flux of sensations inspired Marinetti's stylistic conception of *parole in libertà* ("words in freedom"). From there, it was but a short step to thinking of *parole in libertà* not as a primordial expression of consciousness but as a historically specific expression of modernity. His manifestoes announced "a new religion-morality of speed," a machine aesthetic, and a Nietzschean celebration of danger and war, and communicated them through the rapid movement from noun to noun, the abolition of adjectives and punctuation, and an intoxication with infinitives.⁹⁰ The result was a new code for a new relation between art and public, culture and politics.

⁸⁴ Giovanni Papini and Giuseppe Prezzolini, *Storia di un'amicizia, 1900–1924* (Florence, 1966), 135–36.

⁸⁵ See Mario Richter, *La formazione francese di Ardengo Soffici, 1900–1914* (Milan, 1969), 63.

⁸⁶ Giuseppe Prezzolini, "Le due Italie," *Il Regno* (1904), now in Frigessi, *Leonardo, Hermes, Il Regno*, 500–03.

⁸⁷ Giovanni Papini, *Un uomo finito* (Florence, 1963), 163.

⁸⁸ Giovanni Papini, "La necessità della rivoluzione," in *Lacerba* (1913), now in Gianni Scalia, ed., *La cultura italiana del '900 attraverso le riviste*, vol. IV: *Lacerba, La Voce (1914–1916)* (Turin, 1961), 157–66.

⁸⁹ See P. A. Jannini, "La rivista 'Poesia' di Marinetti e la letteratura francese," *Rivista di letterature moderne e comparate*, 19 (1966): 210–19.

⁹⁰ See De Maria, *Marinetti*, 182–94 and *passim*. In Italian, infinitives often serve as imperative (command) forms, especially in the negative.

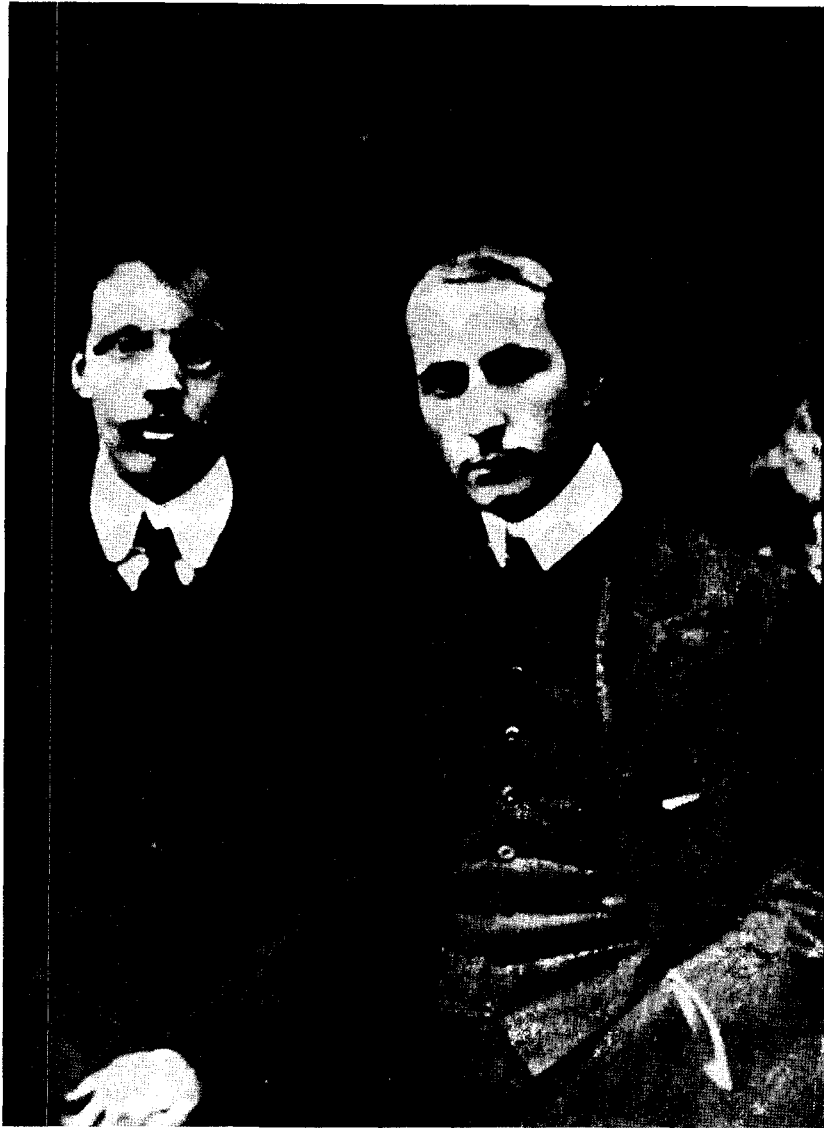


Soffici in 1906 in Paris, where he lived from late 1900 until mid-1907, working as an illustrator for journals such as *La Plume* and *L'Europe Artiste*, and fraternizing with many other artists, including Pablo Picasso. The photograph originally appeared in Giuseppe Prezzolini and Ardengo Soffici, *Carteggio, I: 1907-1918* (Rome, 1977).

But, unlike Soffici and the other *vocianti*, Marinetti had democratic pretensions for futurism. Its various forms of *antipassatismo*—its disdain for the museum, and its shockingly open-ended performances, the famous *serate futuriste* (futurist evenings)—were supposed to sweep away received culture and destroy the old elitist concepts of high culture and detached intellectual. And, as Gramsci later noted, the futurists did achieve surprising strength with the industrial proletariat.⁹¹ Their base in Milan gave them proximity to the syndicalists of *Il Viandante* and to a Milanese socialist-anarchist literary culture that had originated in the 1870s.⁹² Moreover, the futurists were initially much less nationalist than the *vocianti*, even if they were equally committed to cultural renewal and Sorelian regenerative vio-

⁹¹ See Gramsci's 1922 letter to Trotsky on futurism, now in *Selections from Cultural Writings*, D. Forgacs and G. Nowell-Smith, eds. (Cambridge, Mass., 1985), 52-54.

⁹² See Asor Rosa, *Storia*, 989-90, 1295.



Papini and Soffici in 1908. Their friendship intensified over the next half-decade (somewhat at the expense of their relationships with Prezzolini) and culminated in their co-editorship of the journal *Lacerba* from 1913 to 1915. The photograph originally appeared in a commemorative volume, *Papini settant'anni* (Florence, 1951).

lence. Because of this side of its cultural politics, futurism proved difficult to adapt to the cultural needs of fascism in the postwar period and thereby largely unattractive to Mussolini.

The relative superiority of Florentine modernism in this respect was possible only because of its peculiar evolution. *Leonardo* had expressed an initial explosion of energy, its language as flamboyant as its art-nouveau illustrations and irrationalist myth-making. Without sufficient organizational direction, however, it soon deteriorated. After some false starts, Prezzolini decided in 1908 to edit *La Voce*. He himself then became engrossed in Crocean philosophy and syndicalist political thought. But the relative success of *La Voce* resulted not only from the new

seriousness of its editor but also from Prezzolini's ability to let its many talented writers go in their own directions, while the journal achieved a coherent identity around the theme of a new Italian culture. Thus, even as Papini and Soffici became somewhat dismayed by Prezzolini's Crocean and political turns, they themselves turned their literary and artistic energies more to the constructive reappropriation of tradition than to avant-garde creative efforts. Papini supplemented William James with Carducci and reviewed works of history as well as philosophy. Soffici dedicated himself to "educating" Italy about French art: impressionism, Rousseaunian primitivism, and the cubism of Pablo Picasso and Georges Braque.

La Voce appeared weekly from December 20, 1908, to the end of 1913, 263 issues in all, and then in twenty-four biweekly issues in 1914. Although never a great success in terms of circulation, its cultural influence on this and the next generation of Italian intellectuals was second to none.⁹³ Gramsci, for example, later remembered it as having led the pre-war struggle "for a new culture, for a new way of life, and indirectly promoted the formation of original artistic temperaments, since in life there is also art."⁹⁴ As Gramsci's comment suggests, the journal's influence owed much to its modernist character, which was apparent from its inception. The very first issue featured a "letter from London" on George Bernard Shaw, an interview with a German editor, a review of a book on Italian romanticism, and articles on French cultural politics, Georges Sorel, the "ignorance of specialists," and Rudolf Eucken, who that year had won the Nobel Prize for Literature and published *Der Sinn und Wert des Lebens*.

Subsequent issues searched widely for the cultural implications of Italian political issues—the school and railway systems, emigration, regional inequalities, illiteracy, irredentism—and looked literally everywhere for the political meanings of art, literature, music, journalism, and other cultural practices: at the Schlegel brothers, Mozart and Bach, and the "musical crisis in Europe"; at Ralph Waldo Emerson and William James, Robert Browning and Paul Fort, Rimbaud and Verlaine, Henri Rousseau, and Romain Rolland's *Jean Christophe*; at Bergson's view of symbolism and Emile Faguet's of Baudelaire; at Vico, Schopenhauer, Nietzsche, and the "Hegelian Church"; at everyday life in Trieste, Urbino, and the Ticino; at Freud and sexuality, feminism, French and Florentine impressionism, "D'Annunzianism," and the journal *Il Marzocco*; at Henrik Ibsen and August Strindberg, Giovanni Pascoli and Alfredo Oriani; and at Italian psychiatry, the study of grammar, theosophy, and contemporary poetry.

While the pattern of interests was clearly not random, a reader of *La Voce* found no consistent outlook. Rather, the journal saw itself as a kind of intellectual *piazza* in which it was possible to encounter all those ideas about art, philosophy, literature, and life, from the various regions of Italy as well as the various cities of Europe, that promised to contribute to the birth of a genuine modern culture. As Prezzolini explained in 1911, the problem was to overcome the "meagre influence exercised by cultured circles over the development of national politics," to surmount the "strict separation prevalent in Italy between politics and culture,"

⁹³ By the standards of the era, *La Voce's* circulation was not inconsiderable. Prezzolini recalled that it reached 5,000 with one issue, though rarely sold over 2,000 and never had more than 1,000 paid subscribers. See his *L'Italiano inutile*, 180. Yet, when Mussolini launched his daily, *Il Popolo d'Italia*, late in 1914, its circulation quickly reached 80,000. See De Felice, *Mussolini il rivoluzionario*, 278.

⁹⁴ Antonio Gramsci, *Quaderni del carcere*, V. Gerratana, ed., 4 vols. (Turin, 1975), 3: 2190.



Giuseppe Prezzolini at his boyhood home in Perugia around 1905. The photograph originally appeared in Giuseppe Prezzolini and Ardengo Soffici, *Carteggio, I: 1907–1918* (Rome, 1977).

without functioning either as just another mouthpiece for a political party or as a mere literary-critical journal.⁹⁵

This was a triumphant moment for Prezzolini, one at which it seemed he had orchestrated a perfect modernist symphony. In addition to its literary and artistic wing, *La Voce* in 1911 undertook an idealist philosophical enterprise that included the talents of Giovanni Amendola, Renato Serra, Giuseppe Lombardo-Radice, as

⁹⁵ Giuseppe Prezzolini, "La politica della 'Voce,'" now in Angelo Romanò, ed., *La cultura italiana del '900 attraverso le riviste*, vol. III: *La Voce (1908–1914)* (Turin, 1960), 393–94. Prezzolini is explicitly derogatory here in his use of the word "literature."

well as Croce, Gentile, and Prezzolini; a dialogue with Catholic modernism led by Romolo Murri and Giovanni Boine; an economic campaign by Gaetano Salvemini, Scipio Slataper, and Prezzolini to right the imbalances in Italian regional development, as well as a political one on behalf of universal suffrage; and an incipient dialogue with the syndicalists of *Il Viandante*.⁹⁶

After Italy declared war on Libya in September, 1911, however, the symphony became increasingly discordant. Salvemini defected to begin his own more directly political *L'Unità*. Papini also turned toward splinter journals and, though interim editor of *La Voce* in 1912, joined with Soffici in 1913 to begin *Lacerba*, a much more narrowly literary journal aimed at an experimentalist rapprochement with Milanese futurism. "Futurism," he wrote in February 1913, had become "the only avant-garde movement . . . and deserved the sympathy of all those who have not lost themselves in contemplation of the Athos Mountains of history."⁹⁷ But these first heady days of romance gave way to a bitter divorce only two years later.

At the same time, Prezzolini kept his poise under increasing pressure. Indeed, some of his most acute reflective passages appeared in *La Voce* of 1913, including the following one from his provocatively titled "Parole d'un uomo moderno" [Words of a modern man]:

For us the *spiritually* fundamental fact of modern times and of European democracy is the replacement of the social, intellectual, and emotional functions carried out until the French Revolution by the Church. Before the Revolution we had society as a sacred organism dependent on divine will; today we have society as a terrestrial organism dependent on a human will . . .

But while philosophy has overcome the religious-transcendental, modern civilization has not yet succeeded in creating, except as a matter of law, a set of beliefs, a faith, a modern myth . . . The Church has a catechism, democracy does not; the Church has a universal history, democracy does not . . . The malaise of democracy, its crisis, lies precisely here: not in a shortage of religiosity [as the Catholic modernists might maintain] but in a shortage of irreligiosity, in not having sufficiently overcome the religious state and substituted for it some other state ["stato"].⁹⁸

To develop this surrogate secular culture, Prezzolini steered *La Voce* through a self-consciously Gentilian phase until overwhelmed by the onset of the war. Late in 1914, the journal made another abrupt swerve under its new editor, Giuseppe De Robertis, toward a traditional literary criticism *au-dessus de la mêlée*, while Prezzolini turned to politics as Rome correspondent for Mussolini's *Il Popolo d'Italia*.

IF THIS MOMENT MARKED THE END of the Italian modernist project of holding together culture and politics, at least as exemplified by *La Voce*, it also marked a symbolic reversal in the roles of the two protagonists. Until 1914, Prezzolini, older by a year than Mussolini, had been the editor and the teacher; now, in his parting words in *La Voce*, he clearly acknowledged a new role as a subordinate to the editor of *Il Popolo d'Italia*.⁹⁹

⁹⁶ After his *La teoria sindacalista* (1909), Prezzolini made occasional contributions to *Il Viandante*, while the prominent syndicalist writer Agostino Lanzillo contributed two pieces to *La Voce*.

⁹⁷ Giovanni Papini, "Il significato del futurismo" (February 1, 1913), cited in Scalia, *Lacerba, La Voce*, 19.

⁹⁸ See Romanò, *La Voce (1908–1914)*, 519.

⁹⁹ Upon joining *Il Popolo*, Prezzolini wrote in *La Voce*: "Now there is 'Il Popolo.' And I am in Rome to help Mussolini. You know that he is 'a man.' He has made a journal in a week . . . I would hope that

Even though Mussolini was from the beginning much more political than any of the *vocianti*, he nonetheless shared many common experiences and cultural attitudes with his generational counterparts in Florence, and he was not as one-sidedly political as is often thought. Like them, Mussolini had gone abroad in the early years of the century, they to Paris, he to Lausanne (1902–1904). In his autobiography of 1912, he refers to his “bohemian” life there, and he took away enough esteem for French culture to think for a brief period about becoming a professor of French.¹⁰⁰ Like the *vocianti*, too, he was drawn in this period to the philosophy of Nietzsche and the religious socialism of Sorel, both of which he read in Lausanne. Finally, like the Soffici of 1904, Mussolini worried intensely about the social implications of the “death of God,” and his first long essay, finished that July, bore the title *L'uomo e la divinità* [Man and divinity].¹⁰¹

Returning to Italy in November, he soon became a reader of *Leonardo*, which he admired for its generational combativeness and search for a new faith, and from which he seems to have gained an appreciation of William James.¹⁰² But Mussolini himself did not resume the socialist politics and journalistic activity he had begun in Lausanne until the spring of 1908, and his failure to gain steady employment forced his return to his family's house in Dovia that July. The ensuing months were a period of intense reflection during which he wrote a number of pieces for nonsocialist journals. These included an essay on Friedrich Klopstock's poetry during the French Revolution, as well as three reviews on poetry and a long and quite good essay on Nietzsche. In the last, he wrote, “The superman is a symbol, an index of this anguished and tragic period of crisis passing over the European consciousness in its search for new sources of pleasure, beauty, and ideals. He is the recognition of our weakness, but at the same time the hope for our redemption. He is the sunset—and the dawn.”¹⁰³

In 1909, while in the Trentino, Mussolini underwent what one historian has called a “brief but intense exposure . . . to European decadentism, from Baudelaire to Verlaine, Wilde to D'Annunzio.”¹⁰⁴ One evidence of it is the historical novel *Claudia Particella, l'amante del cardinale*, which appeared serially in a Trento journal, *Il Popolo*, from January to May 1910. It was also in Trento that Mussolini became an avid reader of *La Voce*, even helping in its local distribution.¹⁰⁵ In a letter of

many friends of 'Voce,' from the provinces, from the towns, would work with him. Why not inform? They have received from 'Voce' certain guides and instructions. Now it is a question of application"; Giuseppe Prezzolini, "La pagina di Prezzolini I.," in Scalia, *Lacerba, La Voce*, 411.

¹⁰⁰ See Benito Mussolini, *Opera omnia*, 36 vols., eds. E. Susmel and D. Susmel (Florence, 1951–63), 33: 256. In November 1907, Mussolini took up study in Bologna toward a diploma in French, and in the spring and summer of 1908, he taught French in a school in Oneglia.

¹⁰¹ The preface to the essay reads in part: "God does not exist. Religion as knowledge is an absurdity, as practice an immorality, and in men a disease . . . O faithful, the 'Antichrist' is born. The 'Antichrist' is human reason which rebels against dogma and demolishes God"; Mussolini, *Opera omnia*, 33: 3.

¹⁰² See E. Gentile, *Il mito dello Stato nuovo*, 108–09; and De Felice, *Mussolini il rivoluzionario*, 65.

¹⁰³ Santarelli, *Scritti politici di Mussolini*, 109.

¹⁰⁴ Augusto Simonini, *Il linguaggio di Mussolini* (Milan, 1978), 158.

¹⁰⁵ Benito Mussolini, "La Voce," in *Vita trentina* (April 3, 1909), now in his *Opera omnia*, 2: 53–55. De Felice explains Mussolini's attraction well: "La Voce and, still earlier, *Leonardo*, with their bohemianly iconoclastic and regenerative spirit, with their mystical pragmatism, which led them to take aim both against positivism and, in the final analysis, against idealism, with their irrationalism, their thinly veiled 'supermanism' and their critique, of syndicalist and Sorelian stamp, of parliamentary democracy and socialist reformism—[with all these things, the journals] fit Mussolini's psychology well, since he found in their pages the echo of, and the answer to, his aspirations for the radical renewal of Italian society and his aversion, laced with rancor, against bourgeois society and academic culture"; De Felice, *Mussolini il rivoluzionario*, 64–65.



Benito Mussolini at about the time of his founding of *Il Popolo d'Italia* in 1914. The photograph originally appeared in Torquato Nanni's *Benito Mussolini*, published by *La Voce*'s small press in 1915. This extremely celebratory biography is most likely the first to be devoted to the future *duce*.

October 1909 to Prezzolini, he wrote, "*La Voce's* latest initiative is excellent: make Italy known to Italians. Beside political unity, which is slowly but progressively becoming consolidated, it is necessary to forge the spiritual unity of the Italians. This is difficult work, given our history and temperament, but it is not impossible. To create the 'Italian' soul is a superb mission."¹⁰⁶ This modernist project of spiritual renewal is the heart of what Mussolini and Prezzolini shared, and it was clearly what Prezzolini had in mind when he suggested in 1924 that "Mussolini is realizing many of the things that I wanted when I founded and directed *La Voce*."¹⁰⁷

In May 1909, Mussolini reviewed Prezzolini's book on syndicalism for *Il Popolo* quite favorably. He began by noting that Prezzolini did not share his own syndicalist convictions but concluded with a blatantly *La Vocean* view: "By now syndicalism is complete as doctrine; it lacks men. We must make them."¹⁰⁸ From this point until the onset of the war, Mussolini's writing emphasized the political importance of "spiritual elevation," "culture," and "ideals,"¹⁰⁹ so strongly that biographer Renzo De Felice described Mussolini's early socialism as essentially based on "the conjunction of revolutionary Italian socialism with *La Voce*."¹¹⁰

In November 1913, Mussolini founded *Utopia*, a theoretical journal aimed at thinking through the project of cultural renewal and, as such, one of *La Voce's* offshoots. Prezzolini immediately acknowledged its kinship in *La Voce* upon the appearance of *Utopia's* first issue but nonetheless deemed "hopeless" the effort to restore theoretical coherence within the socialist tradition.¹¹¹ In a letter to Prezzolini of March 1914, Mussolini explained that the journal had been founded "in order to be able to find among the younger generation today—socialist and non-socialist alike—the as yet unrecognized intelligentsia capable of reinvigorating theory with new interpretations, whether orthodox or heterodox."¹¹² In its orientation toward youth, its search for political ideas beyond existing ideologies, and its focus on spiritual regeneration, this was a wholly *La Vocean* declaration.

No wonder, then, that when Prezzolini joined the staff of *Il Popolo d'Italia* he managed to bring with him a number of the *vocianti*, including both Papini and Soffici. Indeed, as one historian has written, "whoever glances through the pages of the first *Popolo d'Italia* has almost the impression of moving through an assembly of *La Vocean* signatures."¹¹³ Among other prominent signatures were those of many socialists, syndicalists, anarchists, partisans of the Italian south, and republicans. Conspicuous by their absence were those of any futurists.

Despite the widespread impression that futurism and fascism must have had something to do with one another, Mussolini actually took little from the Milanese avant-garde. In the pre-war period, futurism, with its aestheticism and urban sophistication, had held little appeal for him. In 1914, however, the futurists were among the first to practice an activist *piazza* politics, while more political groups, like the Nationalists, remained hesitant. Mussolini joined them in the *piazza* but still

¹⁰⁶ See Prezzolini, *L'Italiano inutile*, 240.

¹⁰⁷ Prezzolini continued: "Mussolini is one of us. He is a *vociante*"; Renzo De Felice, *Intellettuale di fronte al fascismo* (Rome, 1985), 103.

¹⁰⁸ Santarelli, *Scritti politici di Mussolini*, 114.

¹⁰⁹ See the citations and discussion in De Felice, *Mussolini il rivoluzionario*, 85–87.

¹¹⁰ De Felice, *Mussolini il rivoluzionario*, 284. Mussolini was publicly explicit about *La Voce's* importance for him on at least three occasions: in 1912, 1917, and 1935. See E. Gentile, *Il mito dello Stato nuovo*, 105.

¹¹¹ See Mussolini's reply, "L'impresa disperata (a Prezzolini)," *Utopia*, January 15, 1914: 1–5.

¹¹² Cited in De Felice, *Intellettuale*, 72.

¹¹³ Umberto Carpi, *Giornali vocianti* (Rome, 1979), 190.

developed no relations with them. At the end of the war, the futurists lent key support to Mussolini's *Costituente dell'interventismo* ("Interventionist Assembly") of December 1918 and to his *Fasci di combattimento* ("Fighting Leagues") of March 1919. Marinetti and Mussolini demonstrated together against the moderate Italian Socialist party (PSI) in January 1919. And Mussolini appears to have attended some of their more political *serate* in 1919.¹¹⁴ But these were largely tactical moves aimed at exploiting futurism's considerable appeal among ex-servicemen. While Mussolini occasionally displayed a futurist-style impatience with the Italian fixation on preserving the past in museums and art galleries, he remained too sensitive to the symbolic-political importance of Italy's cultural monuments ever to embrace their uncompromising *antipassatismo*.

ALTHOUGH THE MODERNISTS' APPARENT ABILITY TO INFLUENCE Italy's entry into war in 1915 should have been a triumph for their enterprise, in fact, the controversy over World War I, like the one surrounding the Libyan War in 1911, meant further disarray, which this time spelled the movement's final collapse. Several factors help to explain this turn of events. First, the world war made it clear that the modernists' purely cultural quest of the pre-war years had been utopian and that they would not be the builders of the new culture. While recognizing their need to reach the masses, they had not gone very far in that direction, perhaps because of a wish to maintain themselves as humanists and generalists (as against the "specialized" intellectuals they seemed destined to become). Second, their message reflected their own ideological confusion. With *La Voce*, above all, they had aimed at a national culture based on their own *voce* as an intellectual class aiming to lead a "second Italy." But they had never escaped an essential ambivalence about this goal, for they were both highly critical of Italy and intensely nationalistic. They had never succeeded in clarifying what their "second Italy" would be at the concrete level of institutions, policies, and programs. Third, in their confusion, they had made no alliances, either with the Nationalists on the right or the Socialists on the left, both of whom were too positivist for their taste. Thus their work became only a cultural and intellectual legacy for others more politically shrewd than they.

During the war years, remnants of the modernist avant-garde could be found among three groups: the Gentilians, the Milanese futurists, and the non-futurist literati. The first group included Prezzolini, who distanced himself from fascism in the early 1920s but grudgingly accepted it as "historically necessary" until the murder of Giacomo Matteotti and the resulting crisis of June 1924.¹¹⁵ Most Gentilians had always been more liberal than Prezzolini and, despite their leader, moved decisively into the antifascist opposition by the early 1920s. The futurists became ardent fascists, but they remained somewhat apart, even organizing themselves as an independent political party for a brief period beginning in 1918. In contrast, the non-futurist literati became wholly subordinated to political movements and were immediately faced with a new choice: to cooperate with these movements (like fascism) and ultimately to do their bidding or to return to a more traditional notion of the privatistic, non-avant-garde intellectual. The second

¹¹⁴ For a fuller account, see De Felice, *Mussolini il rivoluzionario*, 474–85.

¹¹⁵ See Prezzolini's well-known debate with Piero Gobetti in 1923 regarding the "società degli apoti," in Giuseppe Prezzolini, ed., *Gobetti e "La Voce"* (Florence, 1971). For the evolution of Prezzolini's relationship with fascism, see De Felice, *Intellettuai*, 62–127.

option generally implied a move toward a classicist aesthetic in seeming repudiation of the modernist avant-garde experience on the level of taste as well as cultural aspiration. The option was adopted with increasing intensity: its first phase came with De Robertis's *La Voce* (1914–1916), a full flowering with *La Ronda* (1919–1923).¹¹⁶ Initially, Papini chose to cooperate, while Soffici in a sense decided to have it both ways—working for *La Ronda* and writing as an ardent fascist. But, over time, both moved increasingly toward apolitical privatism. Papini effectively retired to an exclusively non-public life by 1919, devoting himself to a history of Christ, and Soffici became steadily less the fascist “revolutionary” as the new regime took shape.

Meanwhile, the collaboration and mutual affection of Prezolini and Mussolini remained strong into the 1920s. Sharing the belief that all existing political ideologies were dead, they agreed that some new one was imperative for “forging the spiritual unity of the Italians.”¹¹⁷ But, while Mussolini boldly forged a new “anti-ideology ideology” out of the fascist movement, Prezolini, though recognizing his own ideas in the effort and hoping that it would bring about a cultural renewal, remained too repelled by the movement's style to lend it active support.

Mussolini's debt to *La Voce*an modernism was evident in his fascist pronouncements from 1918 forward, never more so than in his speech at Udine with which this article began.¹¹⁸ Another example can be found in the section of the *Dottrina del Fascismo* that he wrote for the Italian Encyclopedia in 1932.¹¹⁹ Here, he conceded that he had no “doctrine” in 1919, only a “doctrine of action.” Italian fascism had been a “great river” into which had flowed various streams with sources in Sorel, Péguy, and syndicalism. Though unnamed, *La Voce* clearly was one of these streams. Like these streams, fascism knew that “the ideologies of the nineteenth century had been superseded,” and that what was needed was less a “doctrine” than “something more decisive to supplant it—faith.”¹²⁰ Moreover, fascism was “not a party but . . . a movement against all parties”—a statement with resonances in Soffici's *Lemmonio Boreo*, in a famous 1912 article by Croce, and in the most basic self-understanding of *La Voce* that identified “parties” with corrupt, Giolittian Italy and its own non-party of the intellectuals with a “second Italy.”¹²¹ Fascism was a “doctrine of life because it has resuscitated a faith.” It was a “new way

¹¹⁶ *La Ronda* had a predominantly antifascist but slightly ambiguous political profile. Its classicism was clearly a repudiation of modernism—motivated even by the suspicion that modernism had helped produce fascism—and seemed to be a repudiation of fascism. Yet some of its important contributors—like Vilfredo Pareto and Soffici—were either fascists or fellow travelers. The scholarly consensus is that it was, on balance, an antifascist journal, certainly an “apolitical” one. See, for example, Giorgio Luti, *La letteratura nel ventennio fascista* (Florence, 1966), 17–20; Luperini, *Il Novecento*, 1: 321–25; and Ostenc, *Intellectuels italiens*, 208–17.

¹¹⁷ See Prezolini's 1918 meditation on this theme, as cited in De Felice, *Intellettuali*, 83–84.

¹¹⁸ For an analysis of Mussolini's speeches in which his debt to the modernists is unmistakable, see Simonini, *Il linguaggio di Mussolini*.

¹¹⁹ For the original, see the *Enciclopedia Italiana*, 14 (1932): 847–51. For an English translation, see Michael Oakeshott, ed., *The Social and Political Doctrines of Contemporary Europe* (New York, 1950), 164–79. The first part of this entry was written by Gentile, the second by Mussolini. I consider only the second here.

¹²⁰ The distinction between doctrine and faith had been given great currency by Croce in various early political articles, especially “Fede e programmi” (1911), now in *Cultura e vita morale*, 160–70.

¹²¹ When Lemmonio is asked by the local mayor what he means by the statement that he “belongs to no party,” he answers, “It's very simple. I am against the buffoons and thieves whatever party they belong to”; Ardengo Soffici, *Opere*, 7 vols. (Florence, 1959–68), 2: 233. The same point was also argued in 1912 by Croce, though more elegantly, in “Il partito come giudizio e come pregiudizio,” now in *Cultura e vita morale*, 191–98. A year later, Papini made the point dramatically in *Lacerba*: “Irrefutable premise: today, in 1913, an intelligent man cannot belong to any party”; see Scalia, *Lacerba*, *La Voce*, 194.

of life for Italy," one that negated Marxism in just the way Papini had articulated in 1907: fascism "believes . . . in holiness and in heroism, that is in acts in which no economic motive—remote or immediate—plays a part."¹²² It was the spiritual faith that had finally regenerated the culture of Italy.

LIKE MUSSOLINI, THE MODERNISTS generally were from a generation of frustrated intellectuals, desirous of keeping their own threatened social role intact while building a new culture of modernity. They did not really understand what they were building except that, to recall Papini's phrase, it would involve "the preparation of 'spiritual dominion'" on a heroic scale—a new spiritual principle for civilization. In this sense, to employ the metaphor of Andrey Bely's great modernist novel, *St. Petersburg* (1912), pre-war Italian modernism built an ideological bomb that the war and the *biennio rosso* (the "red years" of 1919–1920) set off. Mussolini built fascist culture as an extension—or clarification—of the modernist project. Moreover, this extension was completely plausible since the project itself was so open-ended and politically unspecified, and since its basic impulses and Mussolini's were the same: to spiritualize the world without losing the power of modernity. That is why the modernists never failed to recognize their own image in fascism. As Prezzolini remarked in the 1950s, "Notwithstanding serious differences of method, I could say that in a certain sense Mussolini was the realization of the ideals of *La Voce* . . . Willingly or not, we collaborated in the formation of fascism."¹²³

¹²² *Enciclopedia Italiana*, 14 (1932): 849. For similar language in Papini, see Papini and Prezzolini, *Storia di un'amicizia*, 135–36; and Papini, "Chi sono i socialisti? II. Socialismo e religione," *Leonardo* (March 8, 1903): 1–3, now available in a reprint edition from Arnaldo Forni (Bologna, 1981).

¹²³ Prezzolini, *L'Italiano inutile*, 211.

Renzo De Felice and the Historiography of Italian Fascism

BORDEN W. PAINTER, JR.

RENZO DE FELICE occupies a central and controversial position in the contemporary study of Italian fascism. Some historians hail his voluminous political biography of Benito Mussolini, others think it looks suspiciously like a monument to the Duce. De Felice himself aspires to write a history of Italian fascism based on thorough documentation in order to provide a foundation for future interpretations, but critics charge him with already giving us an interpretation that allows the most favorable reading possible of Mussolini.

The controversy over his work is not confined to academic circles. Periodically, it emerges in the Italian media, as it did in the mid-1970s and again early in 1988. The public attention given to the issues raised by De Felice and his critics marks the depth of the continuing political debate over the place of fascism and antifascism in Italian society. The context of that debate will help us to understand De Felice's work in terms of the historiography of Italian fascism and generic fascism, and it also furnishes an interesting case study in the political use of history.

Here I examine one period of the debate that has stretched over a quarter-century, the years 1974 to 1988. A great controversy broke out in 1974 when De Felice's volume on the years 1929 to 1936, *Mussolini il duce I: Gli anni del consenso*, appeared. In 1988, it re-erupted when De Felice called for an end to the quasi-established antifascism that had undergirded Italian political life since World War II, but that argument quickly became absorbed into a larger debate over Stalinism. By 1988, changes in both the academic and political worlds had created a more receptive atmosphere for De Felice's ideas, although controversy and debate over his methods and his conclusions continued.

THE CENTERPIECE OF DE FELICE'S WORK is the multivolume biography of Mussolini. Five volumes have appeared, bringing the story up to the Italian entrance into World War II on June 10, 1940.¹ The next volume will cover the war years. Finally,

I began this study in a National Endowment for the Humanities Summer Seminar, "Fascism as a Generic Phenomenon," directed by Henry A. Turner, Jr., at Yale University in 1987. I presented a revised version at the Southern Historical Association Annual Meeting in Norfolk, Virginia, in November 1988, on a panel, "Studies of Fascism," with Joseph Biesinger and Anthony Di Iorio, both colleagues in the Summer Seminar. I am grateful for Henry Turner's encouragement and the suggestions of Charles Delzell, Claudio Segrè, Carole Fink, and Frank Coppa.

¹ Published volumes of Renzo De Felice's *Mussolini* are: *Il rivoluzionario 1883-1920* (1965), *Il fascista I: La conquista del potere 1921-1925* (1966), *Il fascista II: L'organizzazione dello stato fascista 1925-1929* (1968), *Il duce I: Gli anni del consenso 1929-1936* (1974), *Il duce II: Lo stato totalitario 1936-1940* (1981), all published in Turin. In preparation is *L'alleato 1940-1945*. For useful summaries of De Felice's work

De Felice promises a one-volume summation with his latest conclusions. He has stressed that his views are always evolving, developing, and changing. "Every book at the very moment it is published, in a certain sense, is rejected by the author, because as he rethinks it he will have something more, something different to say."²

Born in 1929, Renzo De Felice began his academic career as a student of Federico Chabod, whose broad historical interests ranged from the Renaissance to the French Revolution, from nineteenth-century foreign policy to the fascist period. De Felice also acknowledges the influence of Delio Cantimori, another internationally respected Italian historian whose published works deal with historical methodology as well as subjects from the sixteenth through the eighteenth century. Under Cantimori's tutelage, De Felice produced his first publications on eighteenth-century Italian Jacobins.³ De Felice came to the fascist period through a study of the Jews in Mussolini's Italy: *Storia degli ebrei italiani sotto il fascismo* (1961). What began as a diversion turned into a new and permanent direction for his career. Since then, he has been a remarkably productive scholar, concentrating his energy on the study of Mussolini and Italian fascism. Fascism is a political as well as a historical topic in Italy, and De Felice has thus become a public figure. He occupies a place in Italian society unlike that of any scholar in the United States.

Through his first three volumes of the biography of Mussolini, De Felice has challenged the established framework of historiography of Italian fascism. In turn, he has been attacked for both methodology and content. His discussions of Mussolini's intellectual formation and the influences on his thinking, for example, have met with objections that De Felice makes the Duce into a more profound or original thinker than could possibly have been the case. Critics complain that De Felice relies too heavily on documents from within fascism, overlooks evidence that does not fit his views, and too often appears to give Mussolini the benefit of the doubt.

When Volume 4, *Mussolini il duce*, appeared in 1974 with its subtitle, *Gli anni del consenso* [The years of consensus], critics immediately called into question the idea of anything approaching a "consensus" between the fascist regime and the Italian people. De Felice had apparently touched some historical and political nerves. Italians pride themselves on the antifascist consensus that has prevailed in their political life since World War II.⁴ The next year, De Felice took his views to a larger audience by granting a long interview to an American historian. Published in book form (*The Intervista*, 1975), it yielded an easy-to-read 115 pages compared to *Gli anni del consenso*'s 800 pages of text and notes and 100 pages of appendixes. The interview book became a best seller, generating articles in the daily and weekly press, and interviews in the press and on television. Michael Ledeen, who

up to the mid-1970s, see "Introduction," Charles Delzell, trans., Renzo De Felice, *Le interpretazioni del fascismo: Interpretations of Fascism* (Cambridge, Mass., 1977); and "Introduction," Michael A. Ledeen, ed. and trans., Renzo De Felice, *Fascism: An Informal Introduction to Its Theory and Practice* (New Brunswick, N.J., 1976).

² For the comments on a final, abridged volume and the quotation, see Renzo De Felice, *Intervista sul fascismo*, Michael A. Ledeen, ed. (Rome-Bari, 1975): 19–22. [English translation: *Fascism: An Informal Introduction to Its Theory and Practice*, 36–39]. Hereafter, citations of the interview will give page references in both the Italian and English editions as *Intervista* [*Fascism*]. References to Ledeen's intro. to the English edition will appear thus: Ledeen, *Fascism*.

³ *Intervista*, 1–10 [*Fascism*, 21–29].

⁴ See Richard Bosworth, "Italian Foreign Policy and Its Historiography," in *Altro Polo: Intellectuals and Their Ideas in Contemporary Italy*, Richard Bosworth and Gino Rizo, eds. (Sydney, 1983), 65–68.

conducted the interview, summarized the immediate reaction to its publication in July 1975:

By the middle of the month it was the best-selling paperback in the country, and at this writing (mid-October) it is still number one on the best-seller list. It has sold over fifty thousand copies (a remarkable figure in a country with a population of sixty million, an illiteracy rate of over thirty percent, and where only one person out of ten reads a daily newspaper). It has been the object of long diatribes from several of the country's leading intellectuals, and the subject of front-page editorials in the official newspapers of both the Communist and Neofascist parties. It has twice been the subject of prime-time programs on the national television network. Renzo De Felice has been called everything from "soft on Mussolini" to "depraved," and has been accused of trying to "rehabilitate fascism." In short, it is the most controversial book of the year in a highly charged political atmosphere.⁵

Although many historians before De Felice had viewed the years 1929 to 1936—that is, from the Lateran Pact to the conquest of Ethiopia—as the most successful period for the fascist regime, De Felice went further. He found antifascism nearly extinct within the country and thoroughly ineffective from its bases abroad. He argued that, with Mussolini firmly in control of the state as well as the means of communication and education, the fascist government and its charismatic leader had achieved a genuine consensus on fascism among the Italian people.⁶

De Felice had begun his definition of "consensus" in the previous volume of the biography, covering the years 1925 to 1929 (published in 1968).⁷ As evidence, he cited the growing recognition of fascism and Mussolini abroad, Mussolini's prestige at home—which admittedly far outstripped that of the fascist party and fascist ideology—and the growing conservatism of once-revolutionary movements such as futurism, which accorded with a widespread openness to fascism's combination of traditional and revolutionary elements. In fact, part of the regime's appeal was its claim to a revolution that was, first of all, a restoration of the traditional culture and a fulfillment of the Risorgimento.

In *Gli anni del consenso*, De Felice argued that there was both a moral and material consensus on fascism.⁸ The material consensus arose from the security that the regime offered to Italians, including the working class, while the moral consensus grew out of the fascist call for change and a new society that appealed particularly to the young. This moral element represented "that spark of revolutionary fervor that there is within fascism itself, and that tends to construct something new."⁹ The politics of Mussolini's regime ultimately stifled this revolutionary component of the original movement, but in the early 1930s it formed an important part of the consensus.

De Felice qualified his notion of consensus in several ways. First, he placed its strongest point in the period from 1929 to 1934, curiously leaving the Ethiopian war out, and, second, he admitted that the consensus was superficial in character. He also acknowledged that much of its power stemmed from the lack of any alternatives. Because the regime could produce neither the new ruling class nor the new society it had promised, it turned to war in a vain attempt to "fascistize" Italy.

⁵ Ledeen, *Fascism*, 7.

⁶ See Philip Cannistraro, *La fabbrica del consenso* (Rome-Bari, 1976).

⁷ *Mussolini il fascista II: L'organizzazione dello stato fascista 1925–1929*, 369–81.

⁸ See De Felice, *Mussolini il duce I: Gli anni del consenso*, chap. 2, "Gli anni del consenso: Il paese," 54–126.

⁹ *Intervista*, 29 [*Fascism*, 44].

What ultimately led to the demise of Italian fascism was a kind of "self-destruction," stemming in part from the self-deception of Mussolini.¹⁰

De Felice's Duce, for all his cunning and political shrewdness, could not move beyond ideas, policies, and institutions aimed at short-term political benefits. The state came to intervene more and more in national life but often in heavy-handed and inefficient ways, with the Duce trying, but often failing, to keep control. Psychologically, Mussolini turned inward, especially after the death of his brother and confidant Arnaldo in 1931. He isolated himself from the views of others and increasingly came to believe that the "whole edifice of the regime and the destiny of Italy depended on him." While De Felice's Mussolini, then, tried to appear as the infallible Duce who alone knew how to control the masses, he was in fact a kind of behind-the-scenes political broker and mediator who had to keep a number of constituencies sufficiently happy to avoid "putting into crisis the general equilibrium on which the system was founded."¹¹

Mussolini's failure to take full advantage of his opportunities to build a strong foundation for future success also manifested itself in the fate of the fascist party during the 1930s, when Achille Starace served as party secretary. Starace opened up party membership in 1932, requiring civil servants to join and allowing others in who could pay the fee, reversing earlier attempts to restrict membership to a truly fascist elite. Although *staracismo* came to symbolize the virile fascism of physical fitness and daring, it seemed to many a sterile substitute for the revolution fascism had long promised.¹² Mussolini, unable or unwilling to find a way of transforming Italians into new fascist men, turned to a policy of war and empire after 1935, which broke down the superficial and fragile consensus. Thus De Felice allotted about half of his fourth volume to chapters on foreign policy, Ethiopia, and empire.

Before the publication of the *Intervista*, the initial critical appraisals of *Gli anni del consenso* had attacked De Felice's interpretation. Leo Valiani's 1975 review for example, acknowledged the accomplishment of De Felice and the importance of his work for the study of Italian fascism. But it also took issue with De Felice's treatment of Italian foreign policy, arguing that Mussolini's policy up to 1934 was hardly the "diplomacy of peace" that De Felice would have it be. In addition, Valiani offered a more general criticism of the book, one frequently leveled at the biography as a whole: De Felice had fallen prey to that fascination with his subject that often besets biographers. The result was a portrait of the Duce placed in the best possible light. "We do not deny," wrote Valiani, "that Mussolini had his notable personal magnetism, exactly that magnetism that even decades distant still dazzles De Felice."¹³

Such criticism was not new. Roberto Vivarelli had made similar points in his 1967 review of *Mussolini il rivoluzionario*. He took issue with De Felice on a number of specific points, such as his treatment of Italian socialism and Mussolini's place in both the socialist movement and the events of 1912 to 1914. He also made the same larger point about De Felice, namely, that he was dazzled by his subject, did not understand Mussolini's character as an adventurer, and ignored evidence contrary

¹⁰ *Mussolini il duce I: Gli anni del consenso*, 180–81 and following.

¹¹ *Mussolini il duce I: Gli anni del consenso*, 174, 168.

¹² *Mussolini il duce I: Gli anni del consenso*, 216 and following.

¹³ Leo Valiani, *Corriere della sera*, February 7, 1975.

to his views. Above all, Vivarelli complained, De Felice's greatest defect was his lack of any sense of irony in dealing with his subject.¹⁴

De Felice used the *Intervista* to respond to such criticisms and to expand on his tentative conclusions about both Italian fascism and fascism more generally. He admitted that even Mussolini understood the "precariousness" of the consensus he had achieved,¹⁵ but he argued that Mussolini was not as stupid as many wanted to believe. The Duce was aware of the nature of the problems facing him and his regime. De Felice maintained that fascism had a genuinely revolutionary component in its desire to incorporate the mass of the population into the life of the state and the nation.¹⁶ When the precarious consensus did not lead to a true "fascistization" of the Italian people, the regime became more totalitarian and turned to war and conquest.

Citing Jacob Talmon's book, *The Origins of Totalitarian Democracy*, De Felice sought to show that Italian fascism had roots in the left, going back to the French Revolution and Rousseau. In effect, he denied the existence of a generic fascism by arguing that the differences between the Italian and German varieties were greater than their similarities. He did not see the social base of Italian fascism as limited to a lower middle class threatened with a loss of identity and squeezed between the organized working class and the increasing power of big business and industry. Rather, he emphasized the involvement of an "emerging middle class" that in a positive and dynamic way sought entrance into the political life of the nation. Above all, De Felice maintained the importance of distinguishing between fascism as a movement and fascism as a regime. The regime pushed aside but never wholly eliminated the movement, and De Felice sees the movement as the true embodiment of fascism. The distinction between movement and regime is key for him.¹⁷

The *Intervista* also allowed De Felice to talk about his approach to history. Of particular significance was his contention that historical views are always tentative and developing as scholars learn more and uncover new evidence. It is an approach quite different from more structured and theoretical methods that accept certain givens as part of the study of history. His way of doing history, in other words, was bound to clash with the Marxist approaches that have dominated much of Italian intellectual and academic life since World War II. De Felice's continuing polemic with Italian Marxism provides another important clue for understanding where his work fits in the historiography of fascism.

WITHIN THE IMMEDIATE AND WIDESPREAD REACTION to the *Intervista*,¹⁸ the objections to De Felice's views were of two kinds: those that took De Felice to task on specific

¹⁴ Roberto Vivarelli, *Rivista storica italiana*, 79 (1967): 438–58; 458.

¹⁵ *Intervista*, 53 [*Fascism*, 66].

¹⁶ *Intervista*, 63–65 [*Fascism*, 74–76]. Compare Joseph La Palombara, *Democracy Italian Style* (New Haven, Conn., 1987), 273–75, on the legacies and lessons of fascism. La Palombara concluded that "the Fascist party did politicize society! It became the chief mechanism through which people were recruited and promoted to coveted positions"; 274.

¹⁷ *Intervista*, 100, 24–31 [*Fascism*, 102, 40–47].

¹⁸ Some examples include articles and reviews in the following newspapers and magazines, all from 1975: *L'Espresso*, June 29; *Corriere della sera*, July 5 and July 23; *Il Giorno*, July 6; *La Stampa*, July 18; *L'Unità*, July 20; *Panorama*, July 31. Subsequent books that include these and other references and discuss De Felice and the historiography of Italian fascism are: Nicola Tranfaglia, ed., *Fascismo e capitalismo* (Milan, 1976), with essays by P. Alatri, G. Carocci, V. Castronovo, E. Collotti, G. Quazza, G. Rochat, and Tranfaglia; Marina Addis Saba, *Il dibattito sul fascismo: Le interpretazioni degli storici e dei militanti politici* (Milan, 1976); Francesco Perfetti, *Il dibattito sul fascismo* (Rome, 1984); Guido Quazza, et al., *Storiografia e fascismo* (Milan, 1985), with essays by Quazza, E. Collotti, M. Legnani, M. Palla, G.

issues while also calling attention to his allegedly benign attitude toward his subject; and those that went further to reject his general position, his way of doing history, and his presumed political bias. We have already seen the first type of criticism. Leo Valiani wrote two additional articles on the *Intervista* in which he denied the importance of De Felice's distinction between movement and regime and reiterated his disagreement with De Felice's reading of documents and treatment of such issues as foreign policy.¹⁹ Paolo Alatri rejected De Felice's notion of a dynamic emergent middle class, seeing instead a frightened lower middle class on the defensive, "caught between a rising proletariat and the ruling upper middle class, which raised Mussolini to power and kept him there. When has a dictatorship of the Right ever favored the masses? De Felice makes a distinction between the original movement and the actual government. For me, as for everyone else, there is just Fascism pure and simple." Giampiero Carocci commented, "Fascism? A mess, that's all. If we must speak of 'consensus' it is for only the 18 months between the foundation of the Empire and the Spanish Civil War."²⁰

Indeed, it is De Felice's penchant for fine points, details, and distinctions that has particularly annoyed and disturbed those taking a more theoretical and, it might be said, global view of fascism and the forces shaping history. During the controversy in 1975, De Felice offered a revealing analogy: "I feel like a paleontologist trying to reconstruct a dinosaur, and every tiny bone I find fills me with enthusiasm. I have been gathering these bones for 15 years. No one wanted to dig them up, and now everyone wants to send them back. Yet every day I find something new on Mussolini."²¹

Although Alatri and Carocci are Marxist historians, they kept to specific points of disagreement with De Felice and avoided rejecting his work as a whole on ideological or political grounds. Others were not so cautious, and this second type of criticism found De Felice's position, methods, and alleged political bias wholly unacceptable. For example, Giovanni Ferrara, participating in the same interview and discussion with Alatri and Carocci, commented that the approach to fascism had to be "polemical," and he considered De Felice's viewpoint "fascist." Nicola Tranfaglia, a young Marxist historian, charged De Felice with rehabilitating fascism.²² A devastating editorial in the journal of the National Institute for the History of the Movement of Liberation in Italy, *Italia contemporanea*, bore the pejorative title "An Afascist Historiography for the 'Silent Majority'" and denounced positions that ended by becoming "objectively philo-fascist" and thereby having a "diseducational" function. From its perspective, De Felice advanced not only a novel interpretation of fascism but also a new and illegitimate way of doing history. After a brief defense of a Marxist interpretation of fascism, the editorial condemned De Felice and his followers in no uncertain terms. In both his biography of Mussolini and his journal, *Storia contemporanea*, De Felice was guilty of a phony kind of objectivity that refused to take a clear position, placed all theses on

Santomassimo, and an extensive bibliography. Nicola Tranfaglia, *Labirinto italiano: Il fascismo, l'antifascismo, gli storici* (Florence, 1989), is a collection of Tranfaglia's essays and book reviews from 1970 to 1988.

¹⁹ Leo Valiani, *Corriere della sera*, July 5 and July 23, 1975. Valiani later wrote a longer review of both the *Intervista* and *Gli anni del consenso* in *Rivista storica italiana*, 88 (1976): 509-30.

²⁰ Both comments appeared in an interview published in the weekly news magazine *Panorama*, July 31, 1975.

²¹ Renzo De Felice, *Panorama*, July 31, 1975.

²² In *Il Giorno*, July 6, 1975, as quoted in Ledeen, *Fascism*, 17.

the same plane, failed to make any qualitative distinctions between various political, social, and economic forces. The editorial concluded by condemning such academic opportunism as representative of one of the worst traditions of Italian intellectuals.²³

The public debate over De Felice's views quieted somewhat when a senior and revered member of the Italian Communist party, Giorgio Amendola, commented that, although he disagreed with De Felice, a fresh examination of antifascism was needed.²⁴ In effect, Amendola admitted that the fascist regime did forge something of a consensus because antifascism failed to establish a base among all groups and classes of Italian society.²⁵

Not surprisingly, De Felice's views on Mussolini and fascism did not raise in other countries the storm that thundered through Italy in 1975. Professional historians took note, especially in England, where Denis Mack Smith, dean of British historians of modern Italy, managed to make some of the issues public in the pages of the *Times Literary Supplement*. He reviewed *Gli anni del consenso* and the *Intervista* in the TLS of October 31, 1975, taking vehement exception to De Felice's portrait of Mussolini as one far too kind, flattering, and indulgent. A few years later, he summed up his views on Mussolini in a masterful, tightly organized, and lucidly written biography of Mussolini.²⁶

Mack Smith found it "hard to avoid the impression that Mussolini has been given the benefit of too many doubts. We are informed, for example, that he was not cruel (also, incidentally, that he lacked the cold fanaticism of Winston Churchill)," when there is abundant evidence to the contrary. De Felice, according to Mack Smith, simply ignored evidence that did not support his views and expressed views without offering evidence. Where, for example, were the facts to show that fascism was a truly revolutionary and progressive movement? The evidence for De Felice's consensus argument was far from conclusive, and the heavy documentation De Felice employed relied too much on fascist documents. Mack Smith did credit De Felice with showing that the corporative system was a sham, but he complained that

²³ "Fare la storia senza prendere posizione, riferire diverse ipotesi interpretative per negarle tutte, senza peraltro riuscire ad esprimere una propria, ecco il falso modo di problematizzare tipico di questa storiografia. Porre tutte le tesi sullo stesso piano, livellare tutte le forze politiche e sociali—la burocrazia, la diplomazia, le forze economiche, il partito fascista—quasi che tra di esse non vi fosse un differenza qualitativa di peso specifico, ecco un altro dei canoni metodologici della storiografia afascista, esemplarmente rappresentata dalla biografia mussoliniana del De Felice e dalla rivista *Storia contemporanea*, che proprio per le sue caratteristiche promette di offrire larga copertura al mimetismo culturale e all'opportunismo accademico delle peggiori tradizionali intellettuali italiane"; *Italia contemporanea*, 119 (June 1975): 3–7.

²⁴ Giorgio Amendola's comments appeared on the front page of the Sunday edition of the Communist party daily *L'Unità*, July 20, 1975. Subsequently, he was interviewed on antifascism by the historian Piero Melograni: see Amendola, *Intervista sull'antifascismo*, Piero Melograni, ed. (Rome-Bari, 1976). This *Intervista* had little more to say about De Felice's views, but it did explore the subject of antifascism from the 1920s to the 1970s.

²⁵ De Felice complained in the *Intervista* that his critics had overlooked the fact that some of his views agreed with those of Communist leader Palmiro Togliatti: "Years ago—when I wrote my first volume, *Il Rivoluzionario*, and certainly the first volume of *Il Fascista*, and maybe even when the second had appeared—Ernesto Ragionieri published Togliatti's *Lessons on Fascism*. In these *Lessons*, which I could not have known about when I wrote my books, I found certain of my central themes about fascism. No one, even in passing, noticed this 'strange' fact"; *Intervista*, 113 [*Fascism*, 113]. For his assessment of Togliatti's ideas, see De Felice, *Interpretations of Fascism*, 150–51 (orig. pub. in the Italian edition of 1969). Giorgio Rochat charged, nevertheless, that De Felice had misused Togliatti and quoted him out of context; Rochat, "Il quarto volume della biografia di Mussolini di Renzo De Felice," *Italia contemporanea*, 27 (1976): 96.

²⁶ *Times Literary Supplement*, 3842 (October 31, 1975): 1278–80; Denis Mack Smith, *Mussolini, A Biography* (New York, 1982).

then De Felice turned around and called the conquest of Ethiopia Mussolini's "political masterpiece and greatest success," which Mack Smith believed was "worse than shocking, it is nonsense."²⁷

Michael Ledeen then stepped forward to defend De Felice in the *TLS* of January 9, 1976.²⁸ De Felice had, Ledeen maintained, shown Mussolini's failure as a leader, and his calling the Ethiopian war a "political masterpiece" must be seen within the context developed by De Felice; it should not be read as approval of the deed. Ledeen also offered the context for the distinction between movement and regime and the meaning of "revolutionary" as applied to Italian fascism.²⁹ Mack Smith would have none of it. His stinging reply in the *TLS* one week later reiterated that De Felice's heavy reliance on fascist documents had "led [him] to a lopsided view of events." He charged that De Felice used the term "revolutionary" in "a highly idiosyncratic way," and he dismissed Ledeen's discussion of the "political masterpiece" as a misleading characterization of "a catastrophic blunder and as a natural product of the kind of regime Mussolini had so expertly set up." Mack Smith concluded with examples of De Felice's distortion of evidence, especially in the chapter of *Gli anni del consenso* dealing with British and European attitudes toward Mussolini and fascism in the early 1930s.³⁰ None of this criticism kept Mack Smith from acknowledging, a few years later, the value of "the substantial study of fascism being written by Renzo De Felice who has done more than anyone to open up the subject to research in the archives."³¹

However we view De Felice's work, no one can deny the impact it has had on the study of Mussolini and fascism. The furor of the mid-1970s brought his work to the attention of more English-speaking historians. A special issue of the *Journal of Contemporary History* in October 1976 devoted to "Theories of Fascism" included several articles on Italy, with those by Ledeen and Piero Melograni touching on some of the themes in the De Felice controversy.³² The American political scientist A. James Gregor remained true to his maverick views on fascism by writing a strong endorsement of De Felice's position in a 1978 review of *Gli anni del consenso* and the *Intervista*.³³ It is not clear that De Felice would subscribe as enthusiastically to Gregor's views, which depict Italian fascism as a "developmental dictatorship" and seek to exclude German Nazism altogether from the fascist camp.³⁴

The final word is not yet in on De Felice's work. Once he completes the last volume of the biography, we can look forward to his one-volume summation, which will surely be translated into English. At that point, there will be a fresh assessment with, no doubt, renewed polemics, but the results should benefit the general enterprise of understanding fascism, regardless of the disagreements on specific

²⁷ *Times Literary Supplement*, 3842 (October 31, 1975): 1278, 1280.

²⁸ Ledeen is an American who first met De Felice in Italy while doing research for his dissertation, later published as *Universal Fascism: The Theory and Practice of the Fascist International, 1928-1936* (New York, 1972). He conducted the famous interview of the *Intervista* and edited the English translation. The success of the *Intervista* led to *Intervista sul nazismo* (Rome-Bari, 1977), in which Ledeen interviewed George L. Mosse [English translation: *Nazism: A Historical and Comparative Analysis of National Socialism* (New Brunswick, N.J., 1978)].

²⁹ *Times Literary Supplement*, 3852 (January 9, 1976): 35-36.

³⁰ *Times Literary Supplement*, 3853 (January 16, 1976): 58.

³¹ Mack Smith, *Mussolini*, xiv.

³² *Journal of Contemporary History*, 11 (October 1976).

³³ A. James Gregor, "Professor Renzo De Felice and the Fascist Phenomenon," *World Politics*, 30 (April 1978): 433-49.

³⁴ See Charles S. Maier, "Some Recent Studies of Fascism," *Journal of Modern History*, 48 (September 1976): 506-21. Maier's review article discussed the work of both Gregor and De Felice, as well as a number of other books on Italian and German fascism.

points. In the meantime, it is the Italian political and cultural scene that provides the context for De Felice's views and the reactions to them. Antifascism was the foundation of the postwar Republic and was written into the Constitution of 1948. All the parties, with the exception of the neo-fascist Movimento Sociale Italiano (MSI), shared an antifascist faith. This antifascism, according to the Australian historian Richard Bosworth, tended to view Mussolini's regime as having been one "of a semi-criminal minority. The 'Italian people' were unwilling victims of Fascist tyranny. When they had the slightest chance they sympathised with or joined the Resistance."³⁵

IN 1975, THE ITALIAN COMMUNIST PARTY (PCI), espousing "Eurocommunism," was on the ascendant, and it seemed quite possible that it would come into a government with the Christian Democrats or join in a coalition with other parties of the left. Hence it was good politics to keep all doors open and not to question perceived historical truths that had direct connections with present political realities. There was a compromise, Italian style, reports Bosworth, "whereby Christian Democracy and its allies dominated the world of 'real power' in Italy; they formed the government, staffed the bureaucracy, and controlled the state-owned industries. The P.C.I. and its allies in exchange were left the world of the intellect which remained a place of immense and, to an Anglo-Saxon, almost incomprehensible prestige." Within that intellectual and cultural world, "Marxism, or rather Gramscism, seemed to have become the governing ideology of the Italian intelligentsia."³⁶

Antonio Gramsci's revision of Marxism stressed the significance of the political superstructure, the special role of intellectuals in the class struggle, and the concept of hegemony in civil society. Although Gramsci's concepts have given many historians a useful and sometimes highly illuminating context within which to work, De Felice has rejected Gramscian categories, thus setting himself on a collision course with most of Italy's intellectual and academic elite. De Felice concluded the *Intervista* with this statement:

Recent attempts to historicize fascism and the Resistance that a politician like Giorgio Amendola has felt the need to undertake are symptomatic of the political and cultural situation in Italy. On the one hand, they illustrate—by counterpoint—the abstractness and cultural conformism of many of our historians; on the other hand, they offer the possibilities of evaluating communist cultural hegemony. From the mouth of a nonconformist, many of Amendola's affirmations would be considered heresies, and the spirit of his analysis would be considered moderate if not downright reactionary, while coming from Amendola they acquire authority and citizenship.³⁷

Four years later, De Felice expressed rather pessimistic views about the state of Italian historiography, charging that Marxist and non-Marxist historians had failed to find common ground for their historical work. He blamed, in particular, the

³⁵ Bosworth, "Italian Foreign Policy," 68.

³⁶ Bosworth, "Italian Foreign Policy," 68. As Bosworth notes, it is more "Gramscism" than Marxism that dominates the thinking of the intellectual and academic elite of the left. Antonio Gramsci's influence on the historiography of Italy, and that of the West generally, has been particularly important. Gramsci helped found the PCI in 1921 and served the party with distinction until his imprisonment by the fascist regime in 1926. His notebooks and letters, written while in a fascist prison for a decade, are a remarkable corpus that has brought fresh perspectives and new life to Marxist thought.

³⁷ *Intervista*, 115 [*Fascism*, 115].

historiography of the New Left, which had exerted such strong influence after 1968. It challenged traditional Marxist historiography and thwarted the revisionist efforts of Marxist historians begun in 1956. Only Giuliano Procacci and Paolo Spriano, according to De Felice, continued such revisionist work. The Marxist historians also had to worry about their "duty of recovering converts for the Communist Party." Under these circumstances, there was little hope for "any defence of a common historicism."³⁸

When the next volume of the biography appeared in 1981, the audience's mood was quite different. *Mussolini il duce II: Lo stato totalitario, 1936-1940* also met negative criticism, but most of the debate remained on a scholarly level, with little of the public controversy of 1975. The storm had clearly abated, and relative calm reigned. The reasons for the difference arose from changes De Felice made, changes within the academic and intellectual worlds, and changes within the Italian political scene. This time, De Felice gave no *Intervista* to reach a wider audience. Circumstances did not create the environment for repeating such a best seller, and De Felice apparently no longer felt the need to seek the same sort of public hearing he had in 1975.

In his fifth volume, De Felice devoted an entire chapter of nearly one hundred pages to "consenso" between 1936 and 1940.³⁹ His use of quotation marks around the term indicated an apparent retreat from its controversial application to a country ruled by a dictatorship. He stated in the first chapter that the fascist regime continued to enjoy broad support in the period under discussion, but the consensus was less solid and more passive than it had been a few years earlier, because of the Spanish Civil War and the introduction of the racial laws in 1938.⁴⁰ Nevertheless, this "consensus" persisted and was intact when Italy entered the war in 1940. Only with defeat impending in 1942-1943 did it finally break down. That breakdown will presumably be analyzed in the next volume.

Anything De Felice wrote was still news in 1981, even if not at the level of 1975. In an interview in the popular weekly magazine *Panorama*, on July 20, De Felice noted that an argument over his views went on but without it being seen as a struggle between "God and Lucifer." He apparently took comfort from the fact that the debate over consensus now focused on its extent rather than its existence.⁴¹ De Felice saw something of a historiographical turning point ("una svolta storiografica") taking place. One of De Felice's more persistent and consistent critics, Gianpasquale Santomassimo, agreed that the climate had changed. He also felt that De Felice had become more cautious than in the past. He went on to review *Lo stato totalitario*, finding fault again with De Felice's over-reliance on fascist documents and his reaching for conclusions not supported by the sources. Indeed, Santomassimo suggested that some of the sources De Felice cited supported conclusions opposite from those he had drawn. Santomassimo also disagreed with De Felice's portrayal of foreign policy from 1936 to 1940, a large portion of the book.⁴²

French historian Michel Ostenc also saw 1981 as initiating something of a new era

³⁸ Renzo De Felice, "Italian Historiography since the Second World War," in *Altro Polo, A Volume of Italian Studies*, Richard Bosworth and Gianfranco Cresciani, eds. (Sydney, 1979), 178.

³⁹ De Felice, *Mussolini il duce II: Lo stato totalitario, 1936-1940*, chap. 2, "Il 'consenso' tra la metà del 1936 e la metà del 1940," 156-255.

⁴⁰ See the review by Charles F. Delzell, "Mussolini and the Totalitarian State, 1936-1940: The De Felice Interpretation," *Italian Quarterly*, 24 (Summer 1983): 101-08.

⁴¹ The statement reads "vede che vi si discute la misura del consenso. Prima si negava che il fatto fosse, semplicemente"; Renzo De Felice, *Panorama*, July 20, 1981, 128.

⁴² Gianpasquale Santomassimo, "Discussioni di Renzo De Felice," *Passato e presente*, 1 (1982): 23-30.

in the historiography of Italian fascism. He observed that, by 1981, there came "the end of the hagiography of antifascism." This change would clear the way for fresh appraisals of fascism. He hastened to add that this development in no way weakened the moral condemnation of fascism. On the contrary, a more objective study of it would help historians better understand fascism as a historical phenomenon and thus oppose its resurgence.⁴³

Apart from critical comments other historians may make about De Felice's work, the extent of his influence emerges in how other historians use his work. For example, the late Giorgio Candeloro's ninth volume of *Storia dell'Italia moderna, Il fascismo e le sue guerre*, appeared in late 1981. Candeloro included in his bibliography and cited in his text all the volumes of De Felice's biography of Mussolini, including the latest volume, *Lo stato totalitario*.⁴⁴ Candeloro wrote as a historian identified with the left and once again took issue with De Felice's reliance on fascist sources, with his inflation of the consensus enjoyed by the fascist regime and consequent deflation of the coercion it used, and his overemphasis on the ideology and psychology of Mussolini, whose decisions thus appeared autonomous, free from the influence of social and institutional factors.⁴⁵ Yet Candeloro's criticism did not keep him from using De Felice throughout his own volume. He cited De Felice more than any other historian. Over seventy footnotes list works by De Felice, the majority citing volumes of the biography. In one sense, there is nothing surprising in Candeloro's stance. He disagreed with certain aspects of De Felice's perspectives and methods but finds his work a rich mine, as do most historians, for his own understanding of the phenomenon of Italian fascism. And the tenor of his criticism and his use of De Felice's work were far removed from the polemical atmosphere of 1975.

Marco Palla's review of *Lo stato totalitario* in *Studi storici* carried a similar tone. He raised the standard objections to De Felice with respect to outlook, bias, use of documents, and attitude toward Mussolini. Nevertheless, he did not reject, out of hand, the notion of consensus in the 1930s but, rather, criticized De Felice's vague definition of it and lack of strong and clear empirical data based on local studies. In a sense, Palla seemed to suggest that there may have been a consensus, but we do not yet have sufficient evidence for it.⁴⁶

The appearance of the journal *Passato e presente* in 1982 was another indicator of a different intellectual and political atmosphere in Italy. The editorial in the first issue justified its publication by arguing that "no other existing journal seems to have—in content and structure—one of the characteristics that is at the base of *Passato e presente*: the explicit will to reflect the historiographical debate, to discuss the orientations of historiography and, if possible, to influence them." Although starting from a leftist, Marxist, and Gramscian position, the journal's editors hoped to transcend narrow or dogmatic points of view in order to look at the full panorama of historiography on the events of the twentieth century.⁴⁷

Passato e presente also proclaimed that it would be international in its representation of contemporary historiography. Immediately following the first issue's

⁴³ Michel Ostenc, "Historiographie du fascisme italien: Examen critique," *Revue d'histoire de la deuxième guerre mondiale*, 139 (1985): 22–23.

⁴⁴ Giorgio Candeloro, *Storia dell'Italia moderna*, vol. 9: *Il fascismo e le sue guerre* (Milan, 1981).

⁴⁵ Candeloro, *Storia dell'Italia moderna*, 9: 495.

⁴⁶ Marco Palla, *Studi storici*, 1 (1981): 23–49.

⁴⁷ *Passato e presente*, 1 (1982): 3–4. The editors are Franco Andreucci and Gabriele Turi. The editorial committee includes Gianpasquale Santomassimo. Among those listed as *collaboratori* are Enzo Collotti, Eugenio Garin, Eric Hobsbawm, and Marco Palla.

editorial were three reviews of De Felice's *Lo stato totalitario*: two by foreigners, Adrian Lyttelton and Jens Petersen, and the third, already mentioned above, by Gianpasquale Santomassimo. While all three found points of disagreement with De Felice's latest work, the tone of their criticism was temperate. As Lyttelton commented, De Felice was producing a work beyond the scale of anything else attempted on the subject. His conclusion was not unlike Candeloro's rejection of certain aspects of De Felice's work and simultaneous use of it as an indispensable source for his own work on the fascist period.

Richard Bosworth has proposed several reasons for the relatively quiet reception of De Felice in 1981–1982 compared with the uproar of 1974–1975. First, De Felice had succeeded in winning support for his argument that fascism had built some kind of consensus. Thus when he “argued that ‘without war and defeat Fascism would not have fallen,’ or that anti-Fascism was not a serious threat to the regime and almost all discontent towards it was pre-political, or that few among the ‘fellow-travelers’ of Mussolini actively wanted his downfall, there was little reaction.” De Felice's rejection of ideological treatments of history, whether Gramscian-Marxist or Catholic, seemed consonant in the 1980s with increasingly popular public views. “It may be accidental, it may be not, that De Felice's own rejection of Catholicism and Communism and defence of an increasingly conservative lay and capitalist ‘third way’ was paralleled by similar developments on the political stage.”⁴⁸

Bosworth noted that about the only historian to mount a full-scale assault on De Felice's newest volume was Giorgio Rochat, a military specialist. Rochat's review appeared in *Italia contemporanea*.⁴⁹ Once again, De Felice's documentation came under attack, and Rochat charged that he relied far too much on the accounts of Galeazzo Ciano and Dino Grandi for his discussion of foreign policy. For Rochat, De Felice's analysis of consensus was as vague and unsubstantiated as ever. He singled out De Felice's use of the term “cultural revolution” as particularly provocative, since the fascist regime had never used the term, and it could only call to mind Mao Tse-tung's China. Finally, Rochat concluded that De Felice offered only a partial revaluation of fascism with no clear line of interpretation and objected to his use of quotation marks as spreading ever more ambiguity about difficult and controversial terms.

Rochat's final paragraph linked De Felice to the mass media, charging that he seemed to have a near monopoly on public radio and television programs on history, as a result of his political connections and his capacity to give to a large segment of the public the history it wanted. Nicola Tranfaglia seconded this point in an article on fascism and the mass media in 1983. Tranfaglia saw the publication of the *Intervista* in 1975 as the initiation of a challenge to “the dominant interpretation in the culture of the antifascist tradition,” which had “opened the way to the possibility of a gradual revaluation of Mussolini and his regime.” The media had ignored earlier scholarly debate, but De Felice's decision to offer a psychological study that explained events in terms of Mussolini's temperament gained the attention of the media.⁵⁰ Three years later, Enzo Collotti argued that De Felice's influence had been amplified by the work of his students and colleagues in De Felice's journal, *Storia contemporanea*, and his exposure on state television, which

⁴⁸ Bosworth, “Italian Foreign Policy,” 70, 71.

⁴⁹ Bosworth, “Italian Foreign Policy,” 70; Giorgio Rochat, “Ancora sul Mussolini di Renzo De Felice,” *Italia contemporanea*, 141 (1981): 5–10.

⁵⁰ Nicola Tranfaglia, “Fascismo e mass-media,” *Passato e presente*, 3 (1983): 135–48.

so constantly sought his historical opinion that the public might conclude he was the only important researcher on the subject.⁵¹

THE CONCERN EXPRESSED OVER THE INFLUENCE of De Felice's views through exposure in the mass media was also a concern over the changing political atmosphere in Italy. By the early 1980s, the PCI occupied a weaker and more ambiguous position on the Italian political spectrum. The fall of the Christian Democratic government in 1981 led to the appointment of the first non-Christian Democratic prime minister, the Republican party leader Giovanni Spadolini. Even more significant was the assumption of the premiership, after Spadolini's seventeen months in office, by the Socialist leader Bettino Craxi. Craxi held office for over three years until March 1987, longer than any prime minister since World War II.

Elections between 1976 and 1987 showed a decline in the electoral strength of the PCI from nearly 35 percent to just under 27 percent. Craxi had succeeded in increasing the Socialist vote in national elections from just under 10 percent to over 14 percent in the same period, and even a Communist political scientist admitted that his party had lost votes to the Socialists in the June 1987 parliamentary elections.⁵² The Socialists, in other words, had fought for an enhanced position on the Italian party spectrum without collaboration with the Communists. The death of the PCI's popular leader, Enrico Berlinguer, in June 1984 brought immense sympathy from the Italian public but only momentarily halted the electoral decline of the party.⁵³

By 1988, the situation for Italian historiography had also changed considerably from that of 1975. De Felice touched off a second public controversy at the beginning of the year by calling for an end to the official antifascism of the Italian Republic, which he argued was now an outdated obstacle to political reform. Among other things, antifascism "was used by the Communists to assume a patina of democracy and they continue to claim legitimacy with it."⁵⁴

The subsequent debate in the media on these remarks⁵⁵ was engulfed by a new public and political fracas over the historical and political implications of Stalinism. Bettino Craxi's Socialist party (PSI) held a two-day conference in March 1988 on the subject of "Lo stalinismo nella sinistra italiana" [Stalinism in the Italian Left]. A number of historians, including De Felice but with the conspicuous absence of Communist historians, discussed and debated the effects of Stalinism on Italy and Europe with particular attention to the role played during the 1930s and 1940s by

⁵¹ Enzo Collotti, "L'Etat totalitaire," *Revue d'histoire de la deuxième guerre mondiale*, 143 (1986): 32.

⁵² See the article in the *New York Times*, June 21, 1987, IV, 2: 3.

⁵³ For an interesting analysis of the decline of the Communist vote and the gains of the Socialists, Social Democrats, Liberals, and Republicans, see Adolfo Battaglia, "New Politics Emerge in Italy," on the editorial page of the *New York Times*, April 4, 1985.

⁵⁴ As quoted in the Sunday edition of the *New York Times*, January 17, 1988, IV. The interview that touched off the controversy appeared in the *Corriere della sera*, December 27, 1987. Shortly thereafter, Leo Valiani and De Felice had an opportunity to state their views on the question of antifascism in *Nuova antologia* (January–March 1988): 167–71. De Felice emphasized the need to distinguish between antifascists and democrats. The two are not the same, and the communists, in particular, must demonstrate their adherence to liberal democratic values. "An antifascist habit no longer makes a democratic monk" [L'abito antifascista non farebbe più il monaco democratico]. The *Corriere della sera* interview of December 27 and a follow-up interview published January 8, 1989, appear in Jader Jacobelli, *Il fascismo e gli storici oggi* (Rome-Bari, 1988), along with comments by twenty-four journalists and historians.

⁵⁵ See, for example, "L'antifascismo è salvo," in *L'Espresso*, January 17, 1988.

the leader of the PCI, Palmiro Togliatti, who had died in 1964.⁵⁶ The political motivation of Craxi and the Socialists was clear enough: embarrass the PCI, undermine Togliatti's historical stature by his association with Stalin and Stalinism, and, further, make the case that the Socialists were the true inheritors of a democratic socialism whereas the Communists were the inheritors of a tradition tainted by totalitarianism.⁵⁷ The conference prompted complaints from Communist party leader Alessandro Natta that it was unfair to attack and try Togliatti in this fashion.⁵⁸

De Felice's place within Italian historiography on fascism has been most affected by the crisis facing the Italian Marxists both politically and historiographically.⁵⁹ Eurocommunism has failed to develop, and the left wing of the PCI is loath simply to meld into "Mediterranean" socialism. The PSI has considerably enhanced its political strength in Italy and now seriously challenges the PCI for leadership of the left. Marxist historians find themselves on the defensive because the debate over fascism has been dwarfed by the debate over Stalinism. The changes in the Soviet Union brought on by Mikhail Gorbachev have revived all sorts of troubling historical questions that Italian Marxist historians previously either ignored or played down by attributing the sins of Stalinism to Stalin himself rather than to his system or its ideology. In addition, although Gramsci remains the patron saint of the left in Italy and elsewhere, some historians have doubts about the precise meaning of his historical ideas and their political implications.⁶⁰

In these circumstances, De Felice's polemical stance toward Italian Marxist historiography has become part of a larger challenge to the hegemony so long enjoyed by that school. The challenge is fundamentally of a twofold character: a rejection of abstract, theoretical approaches to history; and skepticism about traditional Marxist definitions of fascism. This challenge has opened the way to a

⁵⁶ The conference was held on March 16 and 17, 1988. Some of the papers then appeared in the Socialist party journal *Mondoperaio*, April–May 1988. A volume including all the papers, commentaries and discussions appeared a few months later: *Lo stalinismo nella sinistra italiana*, Atti del Convegno Organizzato da Mondoperaio, Roma 16–17 marzo 1988 (Rome, 1988).

⁵⁷ Craxi had begun his campaign to disassociate the PSI historically and politically from the PCI as early as 1976. See "La storia secondo Bettino," in *Epoca*, March 13, 1988. For a Socialist critique of Gramsci, see Luciano Pellicani, *Gramsci e la questione comunista* (Florence, 1976).

⁵⁸ See, for example, "Che errore attaccare Togliatti," in *La Repubblica*, March 19, 1988.

⁵⁹ A sample of recent books on the Marxist "crisis" by Marxists includes: Carl Boggs, *The Two Revolutions: Antonio Gramsci and the Dilemmas of Western Marxism* (Boston, 1984); Carl Boggs, *Social Movements and Political Power: Emerging Forms of Radicalism in the West* (Philadelphia, 1986); Joseph Femia, *Gramsci's Political Thought: Hegemony, Consciousness, and the Revolutionary Process* (Oxford, 1981); John Hoffman, *The Gramscian Challenge: Coercion and Consent in Marxist Political Theory* (Oxford, 1984); Jack Lindsay, *The Crisis in Marxism* (Bradford-on-Avon, Wiltshire, 1981); Ernesto Laclau and Chantal Mouffe, *Hegemony and Socialist Strategy: Towards a Radical Democratic Politics* (London, 1985); Anne Showstack Sassoon, *Gramsci's Politics*, 2d edn. (Minneapolis, Minn., 1987). Also see the comments on Gramsci and the concept of hegemony in H. Stuart Hughes, *Sophisticated Rebels: The Political Culture of European Dissent 1968–1987* (Cambridge, Mass., 1988).

⁶⁰ The following sorts of questions are being raised and debated by scholars interested in Gramsci: What exactly does hegemony mean? Do Gramsci's ideas lead to the dictatorship of the proletariat or a democratic state? Is the distinction between Western and Eastern Marxism accurate? What did Gramsci think of Stalin and Stalinism?

Victoria De Grazia uses Gramsci's notion of hegemony in her fine study of the *Dopolavoro: The Culture of Consent, Mass Organization of Leisure in Fascist Italy* (Cambridge, 1981), and rejects De Felice's consensus thesis: "Consent is sometimes defined as a favorable public opinion, something subject to being ascertained scientifically at the ballot box or by polling. The assumption that such opinions are freely given, if not freely formed, could never be sustained for fascist Italy; nor, as some recent studies of the regime have implied, is it possible to deduce from the personal charisma of the Duce or from the propaganda bombardments of the Ministry of Popular Culture the existence of a generalized consensus in favor of the regime"; 20. The "recent studies" she cites are De Felice, *Mussolini il duce I: Gli anni del consenso*; and Cannistraro, *La fabbrica del consenso*.

more sympathetic attitude toward De Felice's work.⁶¹ De Felice's contribution has been to demythologize and to depoliticize the study of Italian fascism, arguing that Marxist or Communist views have distorted historical perspective on the subject. The Italian Communists, De Felice seems to say, have profited from a kind of "innocence by association." They have monopolized antifascism and hence purveyed historical views in accordance with their best political interests.

De Felice's work supports two major points with respect to the nature of Italian fascism and fascism more generally. First, Italian fascism can only be understood in its dialectic with communism. Fascism's defeat of Marxism was accomplished by a combination of support from conservative and traditional forces as well as attempts to formulate a revolutionary stance of its own while endeavoring to build its own mass base. The Marxists accept only the first part of such a formulation. Second, generic fascism to De Felice has no basis in historical fact⁶² but is a political construct advantageous to Marxist parties and governments, for generic fascism means that fascism can always appear again. If it does, the Marxists, claiming to understand it "objectively," will be in the best position to thwart it.⁶³

In the short run, the current political situation means that De Felice's views now stand a better chance of getting a sympathetic hearing, though not necessarily unanimous applause. The trend seems to be toward a historiography that concentrates more on specific points and less on theory. In the long run, the issues raised by an examination of De Felice's views of Italian fascism and generic fascism must be assessed within the larger context of the methods underlying the study of history in general and fascism in particular. De Felice has succeeded as a historian not because everyone agrees with what he has written but because, as with Jakob Burckhardt on the Renaissance or Geoffrey Elton on Tudor England, everyone who writes on the subject has to take his views into account.

⁶¹ Even those critical of De Felice's position in general and his consensus argument in particular seemed capable, by the 1980s, of making their points more on the basis of empirical research and less on the basis of political and ideological stances. See, for example, Luisa Passerini, "Work, Ideology and Working Class Attitudes to Fascism," in *Our Common History: The Transformation of Europe*, eds. Paul Thompson and Natasha Burchardt (London, 1982): 54–78; and *Fascism in Popular Memory: The Cultural Experience of the Turin Working Class* (Cambridge, 1987). For a sympathetic summary of De Felice's position and its influence, see the article by his student Emilio Gentile, "Fascism in Italian Historiography: In Search of an Individual Historical Identity," *Journal of Contemporary History*, 21 (1986): 179–208.

⁶² De Felice does see enough similarities in the various fascisms to warrant continuing attempts to define the fascist phenomenon: "The task certainly is not easy, but it is my conviction that—thanks above all to the research and new approach given the problem by some scholars and especially by Jacob Talmon, George L. Mosse, Eugen Weber—it is not impossible"; Renzo De Felice, "Il fenomeno fascista," *Storia contemporanea*, 10 (1979): 627–28. Compare *Intervista*, 24, 82 [*Fascism*, 40–41, 89–90].

⁶³ For examples of non-Italian views on the question of generic fascism, see Martin Kitchen's critique of Ernst Nolte, *Fascism* (London, 1976), 44–45; and Tim Mason's quarrel with De Felice and Henry Turner over modernization, "Italy and Modernization: A Montage," *History Workshop*, 25 (Spring 1988): 127–47. The latter article concludes with some unflattering comments about "Bettino Craxi, the Socialist Party leadership and the media-managers arrayed behind them"; 144.

Cremation in Sung China

PATRICIA EBREY

DISPOSING OF THE DEAD arouses feelings of love, dread, anxiety, and pain. Despite these common human responses, different peoples learn to give form to their emotions in widely divergent ways and thus are often repelled by what others do. The Chinese abhorred some customs of their neighbors and ethnic minorities, such as the Tibetan practice of exposing corpses and the Muslim practice of burying without a coffin. They would surely have been just as horrified by the medieval European practice of dividing bodies. Europeans in China were often made uncomfortable by the northern Chinese practice of keeping encoffined bodies in the courtyard for decades or the southeastern Chinese practice of exhuming bodies after a few years to clean the bones and place them in urns.

These culturally constructed emotional responses to the handling of the dead body do not change easily. In the summer of 1988, the Chinese press carried articles showing that the government had not yet succeeded in its forty-year campaign to replace burial with cremation. For instance, the five children of a deputy chairman of a county party committee had within two days of his death buried him in a hillside grave that took up ninety square meters. They did this although party leaders had tried to keep them from removing the body from the hospital and had then visited their home to insist on cremation. After five weeks of pressure from many party and government officials, the family relented, and the water-logged coffin was exhumed as several hundred villagers watched.¹

In this essay, I examine a counter-example in the history of Chinese mortuary practice, a case in which customs did change. Beginning in the tenth century, many people willingly gave up the long-established custom of burying bodies in coffins to follow the practice introduced by Buddhist monks of cremating bodies and either scattering the ashes over water, storing them in urns aboveground, or burying the urn in a small grave. Throughout the native Sung (960–1297) dynasty and its successor, the alien Yüan (1215–1368) dynasty founded by the Mongol conquerors, cremation flourished despite strong objections on the part of the state and the Confucian-educated elite.² Historians of the West have shown that changes in

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¹ "Yen Ju-chan tzu-nü t'u-tsang ch'i fu ying-hsiang e-lieh," *Hang-chou jih-pao*, August 26, 1988; "Yen Ju-chan t'u-tsang i-t'i tso jih huo-hua," *Hang-chou jih-pao*, September 15, 1988. See also Martin K. Whyte, "Death in the People's Republic of China," in *Death Ritual in Late Imperial and Modern China*, James L. Watson and Evelyn S. Rawski, eds. (Berkeley, Calif., 1988).

² Overviews of the history of cremation in China are provided by J. J. M. de Groot, *The Religious System of China* (1892–1910; rpt. edn., Taipei, 1972), 3: 1391–1417; Naba Toshisada, "Kasôhō no Shina ryôden ni tsuite," *Shinagaku*, 1 (1921): 553–58; Hsü P'ing-fang, "Sung-Yüan shih-tai ti huo-tsang,"

mortuary customs are an excellent lens for viewing changing religious ideas and social organization.³ Changes in such practices in China similarly reveal the complex interaction of popular religion, the ideas of the highly articulate, and the state's efforts to regulate behavior.

The spread of cremation marked a fundamental change in the treatment of the dead body because, until the Sung period, the dominant Chinese preference had been to dispose of the dead in ways that would delay decay.⁴ The Chinese not only used very thick wooden coffins, packed tightly with clothes and shrouds, they also built tomb chambers of wood, stone, or brick and took other measures they thought would preserve the body, such as placing jade objects in the coffins or even, in a famous example, dressing the body in jade. The *Record of Ritual*, a Confucian classic put in final form by the first century B.C., repeatedly mentioned the need to settle an ancestor's body, to make it comfortable. The earthly soul (*p'o*), the soul capable of becoming a harmful ghost, was thought to stay near the body. It could be kept happy if supplied with food, drink, and utensils. Given ancient beliefs about the powers of ancestors, survivors stood to benefit from their ancestors' pleasure. From early times, there was also a strong sense that bodies should be buried intact; part of the obligation to ancestors was to protect their bodies for eventual burial. The Confucian school in particular was identified with pious funeral preparations and reverent sacrifices to ancestral spirits for generations afterward.⁵

In ancient times, some critics opposed the expense incurred by elaborate burial practices, yet they questioned the utility of grave goods and chambers, not underground burial in coffins. The fifth-century B.C. critic of Confucius, Mo Tzu, advocated economical burial but still wanted coffins three inches thick. He described as heartless the customs of the I-ch'ü tribe west of China who cremated their dead in the belief that their souls would ascend with the smoke.⁶ Even though many early thinkers argued that the dead were without feelings or consciousness, it was extremely uncommon for them to draw the conclusion that the treatment of

Wen-wu ts'an-k'ao tz'u-liao, 9 (1956): 21–26; Miyazaki Ichisada, "Chūgoku kasō kō," in *Ajiashi ronkō* III (Tokyo, 1976), 63–84; Makio Ryōkai, "Sōdai ni okeru kasō shūzoku ni tsuite," *Chizan gakuho*, 16 (1968): 47–57; Anna Seidel, "Dabi," *Hōbōgin* (Tokyo, 1929–), 6: 573–85; Yang Ts'un-t'ien and Ch'en Chin-sung, "Wo-kuo ku-tai ti huo-tsang chih-tu," *K'ao-ku yü wen-wu*, 3 (1983): 88–95; Sun Po-ch'uan, *et al.*, "Huo-tsang shih Chung-hua min-tsu tzu ku chiu yü ti wei-sheng hsi-kuan," *Chung-hua i-shih tsa-chih*, 17 (1987): 164–67. Chinese scholars writing since 1949 have generally felt obliged to provide a positive historical background for cremation, to show that it had been practiced by Chinese (including "national minorities") in the past and that it was not simply done out of poverty. Japanese scholars have often been interested in cremation in China because of the different history of cremation in Japan, where it never declined after its early introduction by Buddhist monks.

³ See, for instance, Philippe Ariès, *The Hour of Our Death*, Helen Weaver, trans. (New York, 1981); Peter Brown, *The Cult of the Saints: Its Rise and Function in Latin Christianity* (Chicago, 1981), Elizabeth A. R. Brown, "Death and the Human Body in the Later Middle Ages: The Legislation of Boniface VIII on the Division of the Corpse," *Viator: Medieval and Renaissance Studies*, 12 (1981): 221–70; and Thomas Laquer, "Bodies, Death, and Pauper Funerals," *Representations*, 1 (1983): 109–31.

⁴ Here when I refer to "Chinese," I mean the dominant Han Chinese. Cremation was practiced from early times by some of the non-Han peoples now within the political borders of China, most notably the Ch'iang (proto-Tibetan). For the non-Han practice of cremation, see Sun, "Huo-tsang"; and Yang and Ch'en, "Huo-tsang chih-tu."

⁵ On Chinese funerary customs in general, see Liu Shih-chi, *Chung-kuo tsang-su sou-ch'i* (Hong Kong, 1957). De Groot, *Religious System of China*, also provides extensive discussion of the history of various practices.

⁶ *Mo tzu hsien-ku* (Shih-chieh shu-ch'ü reprint), 6, pp. 113, 116. Compare Burton Watson, trans., *Basic Writings of Mo Tzu, Hsün Tzu, and Han Fei Tzu* (New York, 1967), 72, 76. Mo Tzu's point in mentioning the aberrant practices of several foreign tribes was that each group considers its own practices moral and foreigners' immoral.

the dead therefore did not matter.⁷ Chinese ideas about souls, ancestors, and the afterlife began to change even before the introduction of Buddhism,⁸ yet new ideas did not undermine the preference for underground burial in sturdy coffins.⁹ Even ideas of immortality—especially those associated with the Taoist immortality cult—were corporeal.¹⁰ Moreover, knowledge of cremation as a practice of neighboring peoples did not lead to its acceptance.

AT FIRST GLANCE, the history of cremation in China would appear to be a straightforward example of the gradual penetration of Buddhism into Chinese culture and the reassertion of Confucianism in the Sung dynasty and later. Given the implications of this model for our understanding of the role of philosophical and religious ideas in the evolution of Chinese culture, it should not be accepted without close scrutiny. Buddhism was introduced by the second century A.D., but the process of adaptation to Chinese society was a slow one. Buddhism was not fully incorporated into the common people's social and religious life until the T'ang dynasty (618–906).¹¹ If it could be shown that Buddhism led to changes in the ways ordinary people handled the dead, the magnitude of its impact on Chinese culture would be confirmed. Likewise, evidence that the reassertion of Confucianism was responsible for the decline of cremation after the fourteenth century would demonstrate that intellectual and political changes at the top of the social and political hierarchy changed the everyday lives of ordinary people.

To scrutinize the role of Buddhist and Confucian ideas in these changes in mortuary practices, in this essay I attempt to disentangle different levels of ideas and practices. I distinguish Buddhist ideas from the services provided by Buddhist temples and monasteries, and among Buddhist ideas, I distinguish ones derived from Buddhist scriptures from Buddhist cultic practices and from folk Buddhist traditions not canonical in origin. In a similar way, I distinguish neo-Confucian ideas from state actions.¹² Neo-Confucian objections to cremation were based on

⁷ See de Groot, *Religious System of China*, 1: 659–98.

⁸ See Ying-shih Yü, "O Soul, Come Back!—A Study in the Changing Conceptions of the Soul and Afterlife in Pre-Buddhist China," *Harvard Journal of Asiatic Studies*, 47 (1987): 363–95; Michael Loewe, *Chinese Ideas of Life and Death* (London, 1982), esp. 25–37; Anna Seidel, "Traces of Han Religion in Funeral Texts Found in Tombs," in *Dōkyō to shūkyō bunka* (Tokyo, 1987).

⁹ Some Taoist ideas seem conducive to cremation, but it is difficult to assess whether these are truly indigenous or inspired by Buddhism. The *Lieh Tzu*, a text late enough to incorporate stories influenced by Buddhism, quotes a sage declaring that he was indifferent to the fate of his body after death: it could be burned, thrown into the water, buried with or without a coffin (A. C. Graham, *The Book of Lieh-tzu* [London, 1960], 143). Makio Ryōkai, "Kasō shūzoku," suggested that Huang-lao Taoism had since the Han favored quick decay but offers only a few examples to support this idea. After the introduction of Buddhism, however, Taoism may have indirectly facilitated the spread of Buddhism. Medieval Taoists talked about "deliverance from the corpse" (*shih chieh*); see Isabelle Robinet, "Metamorphosis and Deliverance from the Corpse in Taoism," *History of Religions*, 19 (1979): 37–70. Although this term was used metaphorically to refer to the state achieved in meditation, less educated followers may have taken it literally and thought that being released from one's corpse as rapidly as possible after death had benefits.

¹⁰ See Max Kaltenmark, *Lao Tzu and Taoism*, Roger Greaves, trans. (Stanford, Calif., 1969), 117–21; and D. T. Overmyer, "China," in *Death and Eastern Thought*, Frederick H. Holck, ed. (Nashville, Tenn., 1974), 198–225.

¹¹ For surveys of the history of Buddhism in China, see Kenneth K. S. Ch'en, *Buddhism in China: A Historical Survey* (Princeton, N.J., 1964); and *The Chinese Transformation of Buddhism* (Princeton, 1973).

¹² Studies of mortuary practices and ancestral rites in modern times include de Groot, *Religious System of China*; Emily Ahern, *The Cult of the Dead in a Chinese Village* (Stanford, Calif., 1973); and Watson and Rawski, *Death Ritual in Late Imperial and Modern China*. Studies in folk religion are very numerous. Good places to begin are Arthur Wolf, ed., *Religion and Ritual in Chinese Society* (Stanford, 1974); David K.

sectarian opposition to Buddhism, on belief in the transformative powers of adherence to ancient ritual protocols, and on deeply felt ideas about respect for ancestors. The Sung, Yüan, and Ming (1368–1644) governments sporadically took stands against cremation, issuing laws, urging enforcement of them, and providing alternatives. It is necessary to ask, therefore, whether cremation declined because it came to be thought of as un-Confucian, because it was suppressed, or for other reasons.

In both the spread of cremation and its later decline, ideas from outside these two elite traditions were also important. From studies of Chinese folk religion and mortuary practice in modern China, it is evident that the handling of the dead did not simply belong to the realms of Buddhism or Confucianism; it also involved geomancers, diviners, shamans, and notions of souls and the afterlife better labeled folk, indigenous, or Taoist than either Buddhist or neo-Confucian.¹³ Throughout the T'ang (618–907) and Sung periods, indigenous religious life was strong and vital and not always readily reformed by the efforts of elites, whether Buddhist or Confucian. New cults, such as those of Ma Tsu, Wen-ch'ang, and city gods, were spreading over wide areas of China.¹⁴ Archaeological and literary evidence suggests significant changes in mortuary practice besides the spread of cremation, especially a decline in the use of grave goods, increased delays in burials, and expansion of the cult of the grave. Thus the history of cremation has a context that includes folk religious ideas about bodies, souls, and graves.

This essay pursues the social, religious, and political context of the history of cremation by examining in turn how common cremation became in Sung times, why people turned to its practice, how and why authorities attempted to suppress it, and when it declined. The sources, though not adequate to answer these questions as conclusively as one would like, do show that these changes in customs were considerably more complex historical phenomena than the simple Buddhism–neo-Confucianism model would suggest. Expediency seems to have motivated the choice of cremation as much as belief in its superiority; the ideas that facilitated its acceptance seem to have come as much from the folk level as from Buddhist doctrine; and political efforts to suppress cremation owed as much to anti-foreign sentiments as to anti-Buddhist ones.

THE INCIDENCE OF CREMATION can be judged from both literary and archaeological evidence. Beginning in 962, when cremation was first outlawed, sources contain

Jordan, *Gods, Ghosts, and Ancestors* (Berkeley, Calif., 1972); and Hill Gates and Robert P. Weller, eds., "Symposium on Hegemony and Chinese Folk Ideologies," *Modern China*, 13 (1987).

¹³ On folk religion after the introduction of Buddhism through the Sung period, see the articles in *Facets of Taoism*, Holmes Welch and Anna Seidel, eds. (New Haven, Conn., 1979), especially Rolf A. Stein's "Religious Taoism and Popular Religion from the Second to the Seventh Centuries"; and in *Tantric and Taoist Studies in Honour of R. A. Stein*, Michel Strickman, ed., vols. 2 and 3 (Brussels, 1983, 1985). See also David Johnson, "The City God Cults of T'ang and Sung China," *Harvard Journal of Asiatic Studies*, 45 (1985): 363–457; Stephen F. Teiser, *The Ghost Festival in Medieval China* (Princeton, N.J., 1988); and Valerie Hansen, *Changing Gods in Medieval China, 1127–1276* (Princeton, 1989).

¹⁴ The literature on neo-Confucianism is very large. A good sense of the range of current scholarship can be found in Wing-tsit Chan, ed., *Chu Hsi and Neo-Confucianism* (Honolulu, 1986). On Ming-Ch'ing orthodoxy, see the essays in K. C. Liu, ed., *Orthodoxy in Late Imperial China* (Berkeley, Calif., 1990), especially Liu's introduction.

repeated references to cremation as a widespread practice in China proper.¹⁵ In the early eleventh century, Chia T'ung (*ca.* 1020) wrote that cremation had already become the custom among common people and was gradually contaminating the practices of the educated.¹⁶ In the mid-eleventh century, Han Ch'i (1008–1075) found that, in central northern China, “customs are confused with those of the Ch'iang and Hu [barbarians]. When people die they are cremated and afterwards buried, though the poor deposit the bones in Buddhist shrines, where they accumulate for years in untold numbers.”¹⁷ In 1157, Fan T'ung wrote in a memorial, “Today the people have the custom of ‘transformation by fire.’ While their parents are alive they worry only that they cannot provide everything they need, but when they die they roast them and throw them away . . . Some go so far as to toss the remains in water after burning them . . . Nowadays the cruelty of cremation increases every day.”¹⁸ In approximately the same period, Hung Mai (1123–1202) wrote: “Once the Buddhist theory of transformation by fire arose, everywhere there have been people who burn the corpse on death. When the weather is hot, out of dread of the foul secretions, they invariably lay out [the body] before the day is over and burn it before the flesh is cold.”¹⁹ In 1261, Huang Chen (1213–1280) described a crematory outside the large city of Soochow: “There is a temple called ‘Aid for All’ one *li* to the southwest of the city. For a long time this temple has had about ten hollow structures for cremating people which it operates to make a profit. All the ignorant people of the city are attracted to it; as soon as their parents die, they cart them off and consign them to the flames. The ashes that are not consumed are collected and thrown in a deep pool.”²⁰

Not long afterward, Marco Polo spent seventeen years in China (1275–1292). He recorded the use of cremation alongside other Chinese funerary practices—wearing hemp clothes, keeping coffins in the house for long periods, setting food each day in front of them.²¹ He referred to cremation as the common Chinese way of dealing with the dead and explicitly mentioned its practice in thirteen of the cities he visited, especially ones in coastal provinces.²²

Although critics of cremation often used hyperbolic language, stating that “everyone,” or “all the poor,” or “even filial” sons and grandsons, practiced cremation, it does seem likely that, within a given area, the poor more frequently chose cremation than did the educated, especially the urban poor. Jung I, a central government official writing in 1158, mentioned the difficulty the poor had in finding land for burial in the immediate environs of cities. “Your subject has heard that in Wu and Yüeh [the southeast] the custom is for burial expenses to be so great that one must save up before proceeding. Poor, lower class families have to make every effort to keep funeral preparations economical. For these reasons for some time many of them have found cremation convenient. Through practice it has become the custom, which is thus difficult to change. Furthermore, as the local

¹⁵ In Chinese sources, cremation is referred to variously as *huo-hua* (transformation by fire), *huo-tsang* (fire burial), *shao-shen* (burning the body), *fen-jen* (burning people), *fen-shih* (burning corpses), and related terms. Sometimes, a transliteration of the Sanskrit term *dhyapayati* is also used, such as *she-p'i*, *she-wei*, and *t'ou-p'i* (for these terms, see Seidel, “Dabi”).

¹⁶ Chu Mu, *Ku-chin shih-wen lei-chü* (Ssu-k'u ch'üan shu edn.), 56: 14a.

¹⁷ Han Ch'i, *Han Wei-kung chi* (Ts'ung-shu chi-ch'eng edn.), 13, p. 202.

¹⁸ *Sung shih* (Chung-hua shu-chü edn.), 125, pp. 2918–19.

¹⁹ Hung Mai, *Jung-chai sui-pi* (Ku-chi ch'u-pan she edn.), 13, p. 374.

²⁰ Huang Chen, *Huang-shih jih-ch'ao* (Ssu-k'u ch'üan-shu edn.), 70: 14b.

²¹ Henry Yule, trans., *The Book of Ser Marco Polo*, 3d edn., revised by Henri Cordier, 2 vols. (London, 1903), 1: 204–05; 2: 191.

²² A. C. Moule, *Quinsai, With Other Notes on Marco Polo* (Cambridge, 1957), 44–45.

authorities have been lax for so long, and the population increases daily, great expanses of land would be needed [to bury them all]. As it is difficult for the officials to acquire land near the cities, some have not set any aside [for graveyards]."²³ A scholar in the late thirteenth century argued that banning cremation was impractical. Every day in the capital, hundreds of people died, and there was not enough space to bury them all.²⁴

Yet cremation was not strictly a custom of the poor or of city dwellers. Many of the cremation graves unearthed by archaeologists contain evidence that their occupants were well-to-do. "The rich" were said to practice cremation routinely in "watery" parts of Chekiang, presumably because burial was difficult there; even "upper households" were said to cremate in some areas of North China.²⁵ Both Chia T'ung and Ssu-ma Kuang (1019–1086) reported that educated families were especially prone to practice cremation when their members died away from home, Ssu-ma Kuang noting that sons and grandsons of officials preferred to burn their parents' bodies in order for the ashes to be returned home rather than allow them to be buried far away.²⁶

A somewhat different impression of the incidence of cremation—at least for the upper and middle class in central and southeastern China in the twelfth century—can be found in the tales of the uncanny and marvelous in Hung Mai's *I-chien chih*. Cremation, followed by scattering or burying the ashes, is referred to in a matter-of-fact manner in many tales, confirming the frequent complaint that people were so accustomed to it that they no longer considered it unusual. Nevertheless, there is no sense that cremation was the dominant or preferred practice among those of middling means or better, or even as common as depositing a coffin in a temple indefinitely. To judge from these tales, those who died away from home and those who had no descendants were more likely to be cremated than fathers or mothers who died at home. Cremation appears to have been particularly likely when both conditions were present, when a child, servant, or concubine, for instance, died away from home.

The incidence of cremation also varied from place to place. Literary evidence of the practice of cremation comes from many parts of China. It was found in north and northwest China,²⁷ in central China,²⁸ in the lower Yangtze region (east central China),²⁹ and along the southeast coast.³⁰ There are references to its practice in

²³ *Sung shih*, 125, p. 2919.

²⁴ Yü Wen-pao, *Ch'iu-chien lu* (in *Sung-jen tsa-chi pa chung* [Taipei, Shih-chieh shu-chü edn.]), 4, p. 126.

²⁵ Chou Hui, *Ch'ing-po tsa-chih* (Ts'ung-shu chi-ch'eng edn.), 12, p. 109; Chang Li-hsiang, *Sang-tsang tsa-lu* (in *Tu-li ts'ung-ch'ao*, Kuo-chi chi-yao edn.), 1b.

²⁶ Chu Mu, *Shih-wen lei-chü*, ch'ien 56: 13a–14b; Ssu-ma Kuang, *Ssu-ma shih shu-i* (Ts'ung-shu chi-ch'eng edn.), 7, p. 76. For an example of a Sung official cremated when he died away from home, see Shang-kuan Jung, *Yu-hui t'an-ts'ung* (Pai-pu ts'ung-shu chi-ch'eng edn.), 2: 4a–b. Unfortunately, epitaphs cannot be used to judge how common cremation was in such cases, because sometimes when a tomb inscription survives for a burial of cremated remains, it does not mention the cremation. For an example, see "Ch'üan-chou, Nan-an fa-hsien Sung-tai huo-tsang mu," *Wen-wu*, 3 (1975): 77–78.

²⁷ Han Ch'i (1008–1075) found cremation prevalent in P'ing-chou (Shansi) (*Han Wei-kung chi*, 13, p. 202); Fan Ch'un-jen (1027–1101) in T'ai-yüan (Shansi) (*Sung shih*, 314, p. 10289); Ch'eng Hao (1032–1085) in Chin-ch'eng (Shensi) (*Erh Ch'eng chi* by Ch'eng Hao and Ch'eng I [Chung-hua shu-chü edn.], wen-chi 11, p. 633); and Pi Chung-yu (1047–1121) in Ho-tung (Shansi) (Chang Li-hsiang, *Sang-tsang tsa-lu*, 1a–2a).

²⁸ P'an Chih (1126–1189) found it in Ching-hu-pei (Hupei) (Chu Hsi, *Chu Wen-kung wen-chi* [Ssu-pu ts'ung-k'an edn.], 94: 3b).

²⁹ Jung I (ca. 1150s) reported it as prevalent in the entire Wu-Yüeh area (roughly Kiangsu, Chekiang, and Fukien) (*Sung shih*, 125, p. 2919), Wang Yüeh (1237) in Ch'in-ch'uan (Kiangsu) (Lu Chen, *Ch'in-ch'uan chih* [Sung-yüan ti-fang chih ts'ung-shu edn.], 1: 24a–28a); and Huang Chen (1213–1280)

major cities such as Loyang in the Northern Sung, Hangchow in the Southern Sung, and Peking at the beginning of the Yüan.³¹

Archaeological evidence is particularly good for evaluating the geographical spread of cremation but is naturally limited to cremations that resulted in the burial of urns or boxes of ashes. Moreover, because of variations in climate and soil conditions, not all graves that were dug in Sung times had an equal chance of surviving to be discovered by modern archaeologists. Nevertheless, archaeologists have found Sung period graves with cremated remains in every region of China, including the North China provinces of Shansi, Honan, Liaoning, Inner Mongolia, Hopei, the central provinces of Szechwan, Hunan, Kiangsu, and the southern coastal provinces of Fukien and Kwangtung.³² There could well have been particular places where no one practiced cremation, but it does not seem to be the case that cremation was practiced only in one or two areas with special conditions.

The size and shape of excavated cremation graves, like graves for full body burial, varied from place to place and from one century to another. A tomb in Loyang, in the heartland of North China, dating from about 1100, had a square brick chamber 2.2 meters on each side, with a vaulted roof and an arched door connecting it to the sloped path out. The interior was made to resemble the interior of a house, with doors, windows, eaves, and brackets all fashioned out of bricks. This tomb contained the remains of eight people, seven of whom had been cremated and one who had been first buried elsewhere and the bones later reinterred there. Six of the seven cremated remains were in earthenware urns, the seventh in a wooden coffin two meters long.³³ Since Sung tombs most often only had two coffins, for a husband and a wife, it is possible that the two coffins were for a couple (one moved from elsewhere) and the remains in urns close relatives such as children who died young.

More cremation burial sites have been found in Szechwan than anywhere else, perhaps reflecting the mixing of Han Chinese and minority (non-Han) cultures in this western part of China proper near Tibet. In the area around the city of Ch'eng-tu, 80 percent of a hundred or so Southern Sung (1127–1279) graves excavated in the 1950s contained cremated remains. These were brick tombs, vaulted, usually about 2.5 meters long, 1 meter wide, and 1.1 meters high, although from the late twelfth century on, there were also some smaller ones, only 1 meter long and .5 meters wide. These graves commonly included small cups, urns, coins, and mock deeds of ownership; occasionally, they also had figurines, bowls, incense burners, wine bottles, bronze mirrors, epitaphs inscribed on stone, and Taoist incantations.³⁴ A few of these small graves were almost totally occupied by large,

in Wu county (Soochow, Kiangsu) (*Huang-shih jih-ch'ao*, 70: 14b). There is also a reference to a crematorium in a temple outside a small city in Chekiang; Lu Ying-lung, *Hsien-chuang kua-i chih* (Pai-pu ts'ung-shu chi-ch'eng edn.), 15a.

³⁰ Chen Te-hsiu (1178–1235) found it in Ch'üan-chou (Fukien); Chen Te-hsiu, *Hsi-shan wen-chi* (Ssu-k'u ch'üan-shu edn.), 40: 24b.

³¹ Chang Fang-p'ing, *Le-ch'üan chi* (Ssu-k'u ch'üan-shu edn.), 38: 31a; Wu Tzu-mu, *Meng-liang lu* (in *Tung-ching Meng-hua lu wai ssu-chung* [Shanghai, Chung-hua shu-chü, 1962 edn.], 18, p. 294); *Ta Yüan sheng-cheng kuo-ch'ao tien-chang* (reprint of Yüan edn.), 30: 8a (p. 1273).

³² Yang and Ch'en, "Huo-tsang chih-tu," and Hsü P'ing-fang, "Sung-tai mu-tsang ho chiao-ts'ang ti fa-ch'üeh," in *Hsin Chung-kuo ti k'ao-ku fa-hsien ho yen-chiu* (Peking, 1984), 600.

³³ "Lo-yang Mang-lu-chieh ch'ing-li-le i-tso Sung-mu," *Wen-wu ts'an-k'ao tzu-liao*, 1956.11: 75–77.

³⁴ Hung Chien-min, "Lüeh-t'an Ch'eng-tu chin-chiao Wu-tai chih nan-Sung ti mu-tsang hsing-chih," *K'ao-ku*, 1959.1: 36–39, 23.

well-made ceramic figures of soldiers, servants, officials, and animals.³⁵ In this area, cremation must have been the preferred practice, not a tolerated but inferior choice.

A tentative estimate, based on both literary and archaeological evidence, would be that 10 to 30 percent of the people in Sung times were cremated, with the proportion varying by region, period, and circumstances.³⁶

BUDDHISM CLEARLY HAD MUCH TO DO WITH THE SPREAD OF CREMATION. Cremation had been the standard method of disposing of the dead in India, and Buddhist monks practiced it in China.³⁷ Often, the burnt remnants or “ashes” of eminent monks were preserved through burial at the base of a monument (a stupa), a practice that had already gained popularity in India much earlier.³⁸ These burnt bone fragments were treated as relics, replicating the devout handling of the Buddha’s bones as relics of enormous religious potency.³⁹ By T’ang times, the cult of relics drew on a folk Buddhist belief that the bones of Bodhisattvas (Buddhas-to-be), unlike those of ordinary people, were linked in a chain or miraculously mixed with jewels. After the body was burned, the bones could be inspected and the deceased’s status as a Bodhisattva confirmed.⁴⁰ Perhaps because of the desire to examine and preserve these bones, it seems to have been common in China for cremation to leave pieces of bone large enough to be held (and, when scattered, to

³⁵ Liu Chih-yüan and Chien Shih, “Ch’uan-hsi ti hsiao-hsing Sung-mu,” *Wen-wu ts’an-k’ao tzu-liao*, 1955.9: 92–98. The archaeological evidence shows that cremation was not only common in the Sung, for which there are good documentary sources, but also in the Liao and Chin (neighboring non-Han states that held substantial amounts of previously Chinese territory in the north until defeated by the Mongols). Interesting excavations of Liao, Chin, and Yüan cremation graves include Shan-hsi sheng wen-wu kuan-li wei-yuan-hui, “Shan-hsi Hung-chao-hsien fang-tui-ts’un ku-i chih mu-ch’un ch’ing-li chien-pao,” *Wen-wu ts’an-k’ao tzu-liao*, 1955.4: 46–54; Kuang-chou-shih wen-wu kuan-li wei-yüan hui, “Kuang-chou Ho-nan Chien-chia-kang Sung-yuan mu fa-chüeh chien-pao,” *Wen-wu ts’an-k’ao tzu-liao*, 1957.6: 70–73; and I Ch’ing-an, “Liao-yang-shih Ta-lin tzu-ts’un fa-hsien Liao shou ch’ang erh-nien shih-kuan,” *Wen-wu ts’an-k’ao tzu-liao*, 1956.3: 79–80. The last of these reports describes an unusual burial. An urn of ashes was placed in a carefully fitted stone outer coffin shaped like a typical funerary inscription and cover, with an inscription on two sides of the round hole reporting that the deceased, the wife of an official, died at the age of fifty-three and was buried in 1096. This stone case was then placed in a brick tomb a little over 1 meter square.

³⁶ This estimate must be tentative because of the lack of evidence about children. Presumably, about a third of those who died in Sung times died as children and would seem to have been good candidates for cremation. But I have found very little evidence about the disposal of the bodies of infants or young children.

³⁷ Nishiwaki Tsuneki notes that biographies of monks in the T’ang seldom mention that they have been cremated, only that they were buried under a stupa, whereas Sung ones often mention cremation (“Tōdai sōzoku kenkyū josetsu,” *Tōyō gakushutsu kenkyū*, 18 [1979]: 130–53). Some T’ang monks may have been cremated and their ashes buried under the stupa, but it does appear to be the case that Buddhist monks were not always cremated. Po Chü-i, for instance, explicitly states in some epitaphs that a monk’s “whole body” (*ch’üan shen*) had been buried; *Ch’üan T’ang wen*, Tung Kao, *et al.*, eds. (reprint of 1814 edn.), 678: 20b, 25a.

³⁸ See Gregory Schopen, “Burial ‘Ad Sanctos’ and the Physical Presence of the Buddha in Early Indian Buddhism,” *Religion*, 17 (1987): 193–225.

³⁹ Cremation was not the only means of obtaining relics. Sometimes, the whole body of a priest would be preserved, perhaps even lacquered and treated as an image for worship.

⁴⁰ In one anecdote, a pious Buddhist teacher over ninety years old announced that he would die that day and insisted on cremation. His “chain bones” showed he was a Bodhisattva and motivated his neighbors to collect money to build a stupa where his bones could be buried (*T’ai-p’ing kuang chi*, Li Fang, *et al.*, eds. [Chung-hua shu-chü edn.], 101, pp. 680, 682). An epitaph for a twelfth-century monk recorded that, after he died, he was cremated and shining relics found among the burnt bones his disciples collected and buried in a stupa (Lu Yu, *Lu Yu wen-chi* [Kuang-chih shu-chü edn.], 40, p. 249).

fill up ponds).⁴¹ Even when the person cremated was a commoner, people searched for relics. According to one tale, when the maid in an official family was cremated and her ashes returned home, people nearby smelled the fragrance of lotus flowers and came to search for relics among her burnt bones. In another case, when jewels were found among the remnants of a young wife's cremation, her father-in-law paid for a lavish temple to house them.⁴² Thus Buddhist cultic practice placed cremation in a spiritually positive context.

Buddhism provided institutions that undoubtedly played a role in the spread of cremation. All recorded crematoria were run by Buddhist temples, although it was also possible for people to cremate their own dead by constructing a pyre on open ground. Some Buddhist temples provided storage for the burnt remnants, and others had pools of water where they could be scattered. On the other hand, Buddhist faith did not require cremation. Cremation was not considered a crucial ritual act in Buddhist theology; there are no sutras or commentaries arguing that a person's fate after death depends on burial or cremation, or indeed has anything to do with how the corpse is handled.⁴³ Buddhist missionaries, in trying to convert the Chinese populace, told people to give up sacrifices of meat but did not tell them to stop burying their dead. In the early centuries of Buddhist growth in China, Buddhist clergy were not known for their assistance with funerals. Even in the Sung period, when they had taken on this role, Buddhist monks did not refuse to participate in funerals involving full body burial. Not even all monks were cremated. Chinese critics of cremation generally saw it simply as a foreign custom that had accompanied Buddhism. As one wrote, "The Buddha originally was a barbarian [*hu-jen*]; cremation was the custom of his country."⁴⁴ Even within the Buddhist community, cremation was often referred to as the "method of the Western regions."⁴⁵

Despite the lack of concern with cremation in Buddhist doctrine, acceptance of basic Buddhist ideas—especially karma and transmigration of the soul—must have changed the way many people thought of the dead body. These Buddhist ideas provided no support for the ancient idea that the dead could be comfortable or uncomfortable. The dead person was to be reborn into another body; there was no need for the old body to be preserved or its decay delayed. Buddhism taught non-attachment to material things; neither the dead nor the living had reason to be attached to the body after death.

One puzzle in the linkage between Buddhism and cremation is that Sung and Yüan period cremation burials rarely contain evidence of visual or textual links to Buddhism.⁴⁶ Perhaps those motivated primarily by Buddhist faith had their ashes

⁴¹ Chou Hui (1126–ca. 1198) reported that, at Buddhist temples, the bones of men and women would mingle in pools until the monks cleared them out to make room for more (*Ch'ing-po tsa-chih*, 12, p. 109).

⁴² Hung Mai, *I-chien chih* (Chung-hua shu-chü edn.), *chih-ching* 7, pp. 936–37; *san-chih* 7, pp. 1357–58. Lotus flowers were a common symbol of sanctity.

⁴³ I found only one critic of cremation who said that monks linked it to salvation. Ch'e Jo-shui (thirteenth century) reported that monks claimed it would lead to entry to heaven. *Chiao-ch'i chi* (Pai-pu ts'ung-chu chi-ch'eng edn.), 2: 14b.

⁴⁴ Lu Chen, *Ch'in-ch'uan chih*, 1: 25b.

⁴⁵ Nine such references are found in the *Sung kao seng chuan*, conveniently indexed in Makita Tairyō, *Chūgoku kōsoden sakuin*, 7 vols. (Kyoto, 1978), 7: 978, 1168, and 1205.

⁴⁶ One exception is a burial with an inscription dated 1107 found in an area then occupied by the (non-Han) Khitan state of Liao. The inscription on the stone casket referred to karma, and another stone slab had a Buddhist invocation carved on it; Ta-T'ung-shih wen-wu ch'en-lieh kuan, "Shan-hsi Ta-t'ung Wo-hu-wan ssu-tso Liao-tai pi-hua mu," *K'ao-ku* 1963.8: 432–33. Other nearby cremation graves reported in the same article, however, have walls painted with typical scenes, none distinctively Buddhist (though Buddhist murals were well established for temples), and mock deeds that employ

scattered, leaving no remains. But monks are unlikely to have opposed the burial of burnt bones, as their fellow monks were regularly buried in stupas and even thought of as relics. The explanation probably is that the popularity of cremation owed as much to non-Buddhist as to Buddhist ideas.

IN SUNG TALES, THE GHOSTS OF THE DEAD often come back in dreams to tell of problems they are having. These ghosts use popular Buddhist terminology to discuss their situations, speaking of rebirth, hell, and various services, but do not complain that they would be having an easier time if they had been cremated instead of buried. Buddhist services might speed their rebirth, but cremation would not. Ghosts do, however, complain about their bodies being left above ground in coffins at temples and ask to be cremated as an alternative. Consider the following case:

Juan Yüeh, a vice director in the ministry of revenue, came from Chiang-chou (Kiangsi). At the end of the Hsüan-ho period (1119–1125), he was prefect of Chen-chou (Hunan). While there, his daughter-in-law died of illness, and her coffin was temporarily left at Heavenly Peace Temple. When Juan was about to be replaced, he said to his son, "I am old and glad to be able to give up the seal of office and return. But old people are very superstitious, so I will not take the time to bring your wife's body back east for burial. Please ask the monks at the temple to look after it. Some other time you may come to get it." The son did not dare object. That evening, Juan dreamt that his daughter-in-law came to him, bowed, and said in tears, "I have been left in a coffin in a temple. This makes me a guest-ghost, under the control of the guardian god of the temple. Although at times I get to go back, every morning and evening a bell rings and I must quickly go to hear his orders. This is very bitter for me. Unless I now get to go home with you, I will never have a way to escape. If you are worried about being burdened by the wood of the coffin, I beg to be burned and to go as bones. So long as I am speedily interred in the hills, I will have no more regrets." Juan woke up and, moved by the dream, told his son to precede him and take the coffin back for burial in Chiang-chou. That night he dreamt that his daughter-in-law came to thank him.⁴⁷

In another, rather similar case, concerning a nursemaid rather than a daughter-in-law, after the dream the coffin was located and burned, then the ashes returned home and buried.⁴⁸ Anecdotes of this sort seem to indicate a widely held belief that the dead suffered much more from their bones being left to rest in the wrong place than from the method through which they were reduced to bones. If cremation made it easier for a family to take care of the bones, cremation had much to recommend it. What the dead did not like was the all-too-common fate of being left in a coffin unburied. Because, in early times, slow decay had been seen as more comfortable for the dead, this emphasis on bones rather than complete bodies marks a shift in popular attitudes that helped make cremation more acceptable.

The historical origins of this emphasis on bones are not easy to uncover. Perhaps it was fostered by the Buddhist cult of relics. Or it may have originated in regional cultures of the south, where in later centuries bodies were often exhumed to clean

typical geomantic language. In general, visual or textual links to Buddhism in cremation burials were about as rare as they were in other burials.

⁴⁷ Hung Mai, *I-chien chih*, ping 15, pp. 495–96.

⁴⁸ Hung Mai, *I-chien chih*, ping 11, p. 456.

and preserve the bones.⁴⁹ Whatever its roots, in Sung times it was found in northern as well as southern China. In order to bury their relatives together, people would sometimes move the bones of those who had been dead for generations.⁵⁰ Chia T'ung, from northeast China (Shantung), recognized that his contemporaries would not want to bury their relatives far from home, so he proposed an alternative to cremation. When a death occurred far from home, he wrote, "Buy land and bury the body. Set up a shelter and stay there to care for the grave. Wait for a long time to pass, then pack up the bones and return with them."⁵¹ Chia considered it unfilial purposely to injure the body of one's parent by cremation but did not object to handling the bones if the body were reduced to bones by natural processes.

But the significance of ideas about bones can be pushed only so far. The modern Chinese notion that bones are the pure, enduring, patrilineal part of the dead and the flesh the dangerous, unclean, polluting part is difficult to find in Sung sources.⁵² Nor did most people in Sung times choose to rid the body quickly of its flesh. Indeed, one of the many puzzles concerning the popularity of cremation in Sung times is the simultaneous popularity of a practice almost diametrically opposed to it: retaining coffins for years, decades, or even generations without burying them underground. The motivations for this practice are undoubtedly as complex as for cremation. The two practices were alike in that they could be used as a way to transfer to others the bother of handling the dead: bodies could be quickly brought to temples for encoffining and then abandoned there. But the bodies of parents and grandparents could also be left at temples precisely because the place they were to be buried was so important that descendants needed time to grow up, collect resources, or find an auspicious spot.

The ghost story translated above shows not only that bones were the key material substance of the dead but that their burial in the earth was crucial. This idea had ties to the geomancy of graves, the pseudoscience of locating graves in places where the forces of the earth would lead to the comfort of the dead and the worldly prosperity of their descendants. Although the underlying presuppositions of

⁴⁹ Robert Hertz has noted that there is a fundamental similarity between cremation and two-stage mortuary rituals that involve allowing the flesh to decay and then reburying the bones: both allow for the destruction of the impure flesh and the "drying" of the bones; see "A Contribution to the Study of the Collective Representation of Death," *Death and the Right Hand*, Rodney Needham and Claudia Needham, trans. (Glencoe, Ill., 1960), 43.

⁵⁰ For example, in 1041, Shih Chieh arranged the burial of all those who had not yet been buried over the past five generations in his family, altogether, seventy sets of remains. His family had originally come from Hopei but had moved to Shantung about a century earlier; see *Tsu-lai Shih hsien-sheng wen-chi* (Chung-hua shu-ch'ü edn.), 168–69, 188, 234–35. Anthropologists have found that such relocated bones were not uncommon in North China burials.

⁵¹ Chu Mu, *Shih-wen lei-ch'ü*, ch'ien 56: 14a. This is similar to the practice of double burial common in Fukien, Kwangtung, and Taiwan in recent times. There is no evidence, however, of any group practicing double burial as its preferred custom in Sung times, and none of the numerous complaints about vulgar customs and departures from the Confucian ritual classics mention excavating coffins merely to clean the bones and rebury them in the same general locale.

⁵² For these ideas in modern southern China, see James L. Watson, "Of Flesh and Bones: The Management of Death Pollution in Cantonese Society," in *Death and the Regeneration of Life*, Maurice Bloch and Jonathan Parry, eds. (Cambridge, 1982), 155–86. The closest I have come to such sentiments in a Sung source is from a Buddhist advocate of cremation in the early twelfth century who argued that cremation was preferable to burial because it "stops the foulness of smelly decay, avoids consumption by ants, and allows the *p'o* spirit to escape, the soul to become pure and ascend"; Chieh Huan, "Miao-fa lien-hua ching-chieh," in *Wan hsü-tsang ching* (Hsin wen-feng ch'u-pan kung-ssu edn.), 47, p. 337d. Sung scholars did report, however, that their contemporaries had a variety of fears about mourning activities; see Hung Mai, *Jung-chai sui-pi*, 9, 123; Chang Tsai, *Chang Tsai chi* (Chung-hua shu-ch'ü edn.), 301; Ssu-ma Kuang, *Ssu-ma shih shu-i*, 6, pp. 63, 67–68; Yü Wen-pao, *Ch'iu-chien lu*, 124.

geomancy rely on the ancient idea that the living benefit when their ancestors are comfortable, its tenets were greatly elaborated over time, drawing on several strains of cosmological thought and divinatory traditions. Belief in the geomancy of graves pervaded all levels of Sung society. The few surviving geomantic texts of the Sung or Yüan eras refer most often not to bodies or corpses but to bones. The *Ta-Han yüan-ling mi-tsang ching* refers to “planting” bones.⁵³ Another burial classic, the *Tsang-shu*, edited by Ts’ai Yüan-ting (1135–1198), refers not to burying parents’ bodies but to burying their bones. It also refers to the bones as the essence of what people receive from their fathers and states that proper burial is one method of getting “vital force” (*ch’i*) to reenter the bones in order to give protection to those descended from them.⁵⁴

Chinese scholars of the classics frequently argued that the dead could be buried wherever convenient; for ancestral rites, the location of the grave did not matter, since the heavenly soul, the *hun*, could be worshiped anywhere.⁵⁵ Yet most people in Sung times felt strongly that their parents’ and grandparents’ graves should be close enough to visit. Graves were a major locus of ancestor worship at all social levels. Sung literati have left voluminous evidence of their concerns with graves. Some went to great efforts to find old graves, others to preserve graves from encroachment by outsiders, to organize rites to be held there, to erect shrines for these purposes, or to assure the continuance of these rites by endowing them with land.⁵⁶ It was common in Sung times, and even sanctioned by the government, for officials to set up at their ancestors’ graves Buddhist shrines where monks would perform their sacrifices for them.⁵⁷

Graves were so important for settling the soul and providing a place for cultic activity that graves without bodies were sometimes created. When a person drowned at sea or a body could not be found, a grave might be made anyway, with some symbol of the dead person placed in it, to settle the *p’o* soul and provide a place to present offerings.⁵⁸ The significance of graves at every level of Sung society undoubtedly explains why considerable sums were often expended to bury

⁵³ This text is found in chap. 8199 of the *Yung-le ta-tien*. On this text, see Hsü P’ing-fang, “T’ang-Sung mu-tsang-chung ti ‘Ming-ch’i shen-sha,’ yü ‘mu-i’ chih-tu—tu ‘Ta-Han yüan-ling mi-tsang ching’ tsa-chi,” *K’ao ku*, 1963.2: 87–106.

⁵⁴ *Liu Chiang-tung chia-ts’ang shan-pen tsang-shu* (Pai-pu ts’ung-shu chi-ch’eng edn.), 1b–5a, 6b. Although geomancy, through its emphasis on bones, can be seen as compatible with cremation, it also can be seen as in tension with it. Wang Yüeh, in the mid-thirteenth century, claimed that people chose cremation because they were so confused by the theories of geomancers and afraid of the bad consequences of burying in the wrong place that they simply wanted to get rid of the body quickly by throwing it in a fire (Lu Chen, *Ch’in-ch’uan chih*, 1: 25b–26a). Lo Ta-ching, also mid-thirteenth century, cited as evidence against the validity of geomancy the case of a highly successful official whose parents and grandparents had been cremated and their ashes buried; *Ho-lin yü-lu* (Chung-hua shu-chü edn.), ping 6: 345.

⁵⁵ Ssu-ma Kuang, *Ssu-ma shih shu-i*, 7, p. 76, wrote, “When Yen-ling Chi-tzu went to Ch’i, his son died and he buried him in the region of Ying and Po, saying it is fate for the bones and flesh to return to the earth, while the soul goes everywhere. Confucius considered that this conformed to ritual. When one definitely cannot return a body for burial, it is acceptable to bury it in the local place. Isn’t this better than burning it?” (The allusion is to *Li chi*, “T’an Kung” [Shih-san ching chu-shu edn.], 10: 18b, translated in James Legge, *Li Chi* [New Hyde Park, 1967 reprint of 1885 edn.], 1: 192–93). See also *Erh Ch’eng chi*, i-shu 2b, p. 58.

⁵⁶ See Patricia Ebrey, “Early Stages in the Development of Descent Group Organization,” in *Kinship Organization in Imperial China, 1000–1940*, Patricia Buckley Ebrey and James L. Watson, eds. (Berkeley, Calif., 1986), 20–29.

⁵⁷ Chikusa Masaaki, *Chūgoku Bukkyō shakaishi kenkyū* (Kyoto, 1982), 111–45.

⁵⁸ For some Sung examples, see Hung Mai, *I-chien chih*, san-hsin 3, pp. 1407–08. See also Ssu-ma Kuang, *Ssu-ma Wen-cheng kung ch’uan-chia chi* (Wan-yu wen-k’u edn.), 18, pp. 276–77, for Confucian objection to this custom.

cremated remains. When the ashes were scattered, there was no grave. Those who had fully assimilated Buddhist cosmology accepted this and altered their ritual practices accordingly. Ching T'ang (1138–1200), a high official from Kiangsi, came from a poor family, and both his father and grandfather had been cremated. Since there was no grave for them, at the Ch'ing-ming festival Ching offered his sacrifice in an open field.⁵⁹ It was also reported that descendants made offerings by the side of the pool into which they had tossed the ashes.⁶⁰ Others, however, were distressed by the lack of a grave. In the twelfth century, a man whose mother, a concubine, had been cremated in his youth was so troubled by the lack of a grave that he had a wooden statue of her carved, dressed in grave clothes, and buried in a coffin. He then set aside land to pay for regular sacrifices at the empty grave.⁶¹ During the Yüan period, Fu Shou-kuang felt anguish when his elder brothers had his father cremated and disposed of the burnt bones in a large body of water. After his brothers left, he took off his clothes and went into the water to collect what bones he could. He stored them in a case and consulted a geomancer to find a place to bury them.⁶²

Some of the folk stories in the *I-chien chih* imply that scattering the ashes could help to weaken the material basis of a ghost. In one, a courtesan had been cremated by her former patron, who kept her ashes in a wooden box. When this box began moving on its own and making noises, he had the ashes scattered over a river. In another tale, a ghost of a woman who had no surviving descendants was causing trouble to some neighbors. They exhumed the coffin and found that the body had not decayed despite the passage of over twenty years and the disintegration of the coffin. To end her interference, they burned the body and threw the ashes into water.⁶³ Thus scattering ashes was useful when dealing with a resentful ghost rather than a kindly ancestor.

Although these folk ideas show that cremation was a tolerable alternative to burial, especially if the burnt bones were buried, none of them indicate that cremation was the preferred way to dispose of one's own parents. It is difficult to judge how many people chose cremation as the best way to dispose of a body. Many people died away from home (especially because home generally meant where one's ancestors were buried, not where one had lived the last decade or two), and their burials could account for many of the better-furnished cremation graves. The poor who chose cremation may have considered it an inferior way to dispose of their parents, only acceptable because common. Certainly, that is how the educated looked on their actions. The educated did not attribute to the poor an alternative view of the body in terms of which cremation was superior to burial. Nor do advocates of cremation as a preferred method appear in Sung anecdotes and ghost stories.⁶⁴

⁵⁹ *Ho-lin yü-lu*, ping 6, pp. 344–45.

⁶⁰ Chou Hui, Wu Tzu-mu, *Ch'ing-po tsa-chih*, 12, p. 109.

⁶¹ Wu Tzu-mu, *Meng-liang lu*, 15, pp. 260–61.

⁶² Sung Lien, *Sung hsüeh-shih wen-chi* (Wan-yu wen-k'u edn.), 62, p. 1005.

⁶³ Hung Mai, *I-chien chih*, chih-ching 9, p. 948; san-ssu 9, pp. 1375–76.

⁶⁴ It is possible, of course, that such views existed among the illiterate, who have left no sources. One set of ideas that could have contributed to a positive view of cremation concerns the transformative powers of fire. Fire had strong association with the world of spirits, especially with transferring material objects into forms that could reach that world. Burning prayers was standard procedure both in Taoist rituals and in communications with ancestors. Burning incense had become a common way of showing respect to spirits of all sorts, including ancestors. Burning mock paper money was the most common way to transmit offerings to spirits. At funerals in Sung times, it was a popular custom to burn paper replicas of objects, thereby sending them to the deceased. Why not also send off the dead by burning them?

THE TIMING OF THE SPREAD OF CREMATION must be taken into account in analyzing the reasons it gained popularity. By the mid-T'ang dynasty, basic Buddhist ideas were well known in China and cremation a familiar practice of Buddhist monks. By then, monks performed funeral services at regular intervals after death, and the Buddhist ghost festival in the seventh month had become a major occasion for offerings to ancestors.⁶⁵ Yet scholars have found almost no evidence of lay persons being cremated in T'ang times.⁶⁶ Nor have archaeologists found evidence of lay people's cremation burials in native Chinese (Han) areas before Sung times. If cremation was so convenient, received support from Buddhist ideas and institutions, limited contact with unpleasant corpses, and preserved the essential material substance of the dead (the bones), why did it take so long to become common?

One part of the solution to this puzzle may simply be that Buddhism had in fact more deeply influenced family life by Sung times than it had during the T'ang.⁶⁷ No reader of the *I-chien chih* can escape the conclusion that Buddhist monks, temples, and services were ubiquitous in Hung Mai's twelfth-century world. Recent scholarly work reveals many other signs of the vitality of Buddhism in the Sung.⁶⁸ Although, in Sung times, the educated were more often anti-Buddhist than they had been in the T'ang period, monks and temples may have been an even more familiar part of the common people's social landscape than they had been earlier.

A second part of the solution may be that cremation was adopted as much as an expediency as an act of Buddhist piety. Many of those who chose cremation did so not because they believed it was better than burial of the full body but because it was an acceptable second choice. Cremation often seems to have been associated with special circumstances: death away from home or the death of someone who had not had children and thus could not become a focal ancestor. When a man selected cremation as an expediency, it would not matter what he thought of Buddhist

Throughout the T'ang period, monks occasionally immolated themselves as a pious sacrifice in imitation of the Bodhisattva described in the *Lotus Sutra*, attracting huge crowds of observers. See Jacques Gernet, "Les Suicides par le feu chez les Bouddhistes chinoises du V^e au X^e siècle," *Mélanges publiés par l'Institut des Hautes Etudes Chinoises*, 2 (1960): 527–58; and Yün-hua Jan, "Buddhist Self-Immolation in Medieval China," *History of Religions*, 4 (1965): 243–68. Just as the monks who set themselves on fire were seen as offering their bodies, so perhaps in folk thinking there may have been some sense that the bodies of those who died naturally could be "offered" through cremation. This idea was apparently a part of Hindu thinking about cremation. See Jonathan Parry, "Sacrificial Death and the Necrophagous Ascetic," in *Death and the Regeneration of Life*, Maurice Bloch and Jonathan Parry, eds. (Cambridge, 1982), 74–110. The possible connections between cremation and other uses of fire to communicate with spirits are intriguing, but I have found no sign that any of the educated in Sung times attributed such ideas to those who chose cremation.

⁶⁵ Teiser, *Ghost Festival in Medieval China*.

⁶⁶ For one reference to cremation of a lay person in the T'ang, see Miyazaki, "Chūgoku kasō kō," 67. To this could be added the evidence that some societies of lay Buddhists in Tunhuang in the ninth or tenth centuries may have practiced cremation: when a member died, a circular notice would be sent to the others asking for contributions of firewood in addition to grain, oil, and cloth. Whether this wood was used for cooking or for a funerary pyre is unclear; see Naba Toshisada, *Tōdai shakai bunkashi kenkyū* (Tokyo, 1947), 530, 532; and Nagasawa Kazutoshi, "Tonkō no shōmin seikatsu," in *Tonkō no shakai*, Ikeda On, ed. (*Kyōza Tonkō*) (Tokyo, 1980). It should be conceded that an absence of references to cremation is not strong evidence of its absence or rarity. However, one would expect cremation to have at least attracted the attention of scholars who opposed Buddhism and criticized other Buddhist practices.

⁶⁷ Makio makes this suggestion; "Kasō shūzoku," 56.

⁶⁸ See Chikusa, *Sōdai Bukkyō shakaishi kenkyū*; Huang Min-chih, *Sung-tai fo-chiao she-hui ching-chi shih-lun chi* (Taipei, 1988); Griffith Foulk, "The 'Ch'an School' and Its Place in the Buddhist Monastic Tradition" (Ph.D. dissertation, University of Michigan, 1987); and Miriam Levering, "Ch'an Enlightenment for Laymen: Ta-hui and the New Religious Culture of the Sung" (Ph.D. dissertation, Harvard University, 1978).

theology, so long as he was not adamantly opposed to dealing with Buddhist monks. Thus the key contribution of Buddhism may have been to make available Buddhist institutions. This seems to be the case with another Sung funerary practice: depositing coffins in Buddhist temples until burial could be arranged. There was nothing in Buddhist doctrine that encouraged delaying burials, the Indian practice having been very prompt cremation, but Buddhist temples as institutions facilitated this practice. Even those indifferent to Buddhist religion found it convenient to take advantage of this service the temples offered.

If expediency was as important as theology, historical circumstances may have played a major role in the increasing use of cremation. The social and political conditions of the late ninth and tenth centuries probably provided a needed stimulus for the expansion of this practice. The period from 880 to 980 was one of constant warfare and extensive migration. In the north, non-Han peoples, especially the Khitan, pushed farther into Chinese territory. Cremation had advantages for those forced to move. They may well have had coffins awaiting burial stored in some nearby temple. Rather than abandon the coffins of close relatives that had never been buried (it was bad enough to abandon the graves of their ancestors), they could have them cremated and carry the ashes in urns. Those fleeing warfare also faced the need to dispose of casualties; cremation was not only quick but allowed them to bring the ashes along and perform the remaining funerary rites en route.⁶⁹ The three earliest recorded cases of cremation by lay people were of this sort. In the 880s, a military commander, dying in battle, asked to be cremated and have his remains sent home.⁷⁰ In 946, Shih Chung-kuei, a non-Han military man who had taken over as ruler of North China from his father, was defeated by the Khitans. He, his mother, and his father's main wife were exiled to the far northeast. When his mother was about to die, she said, "Burn me to make ashes and scatter them to the south so that what survives of my *hun* soul can return to China." Her body was cremated but the ashes saved. In 950, the widow also became ill, and she said, "When I die, burn my bones and send them to the Fan-yang Buddhist temple. Do not let me become a ghost in a captive land." She was also cremated, and both sets of ashes were then buried.⁷¹

Migration placed people in new surroundings where old customs might no longer seem appropriate. Migrants who ended up in large cities may have found disposing of the dead more complicated than it had been for their parents in the countryside; they may then have been quite willing to use Buddhist crematories. Migrants from the north who fled to the Yangtze River Valley or farther south may have been reluctant to bury the bodies of their parents there because soil conditions were so different from what they had known in the north. The heat and humidity of the south led to problems in keeping bodies in coffins for weeks or months before burial. Ants and ground moisture were much greater problems than in the north, requiring elaborate tomb construction if the body were not to decompose rapidly after burial.⁷² To newcomers unused to these conditions, the thought of their parents' flesh being quickly consumed by fire may have seemed preferable to

⁶⁹ One might challenge my emphasis on disorder, warfare, and migration by pointing to similar conditions in the fourth century that did not lead to the spread of cremation. The difference between these two periods lies very much in the underdevelopment of Buddhist establishments in the earlier period. It was not until the T'ang dynasty that Buddhist establishments came to play much of a role in serving lay people's ritual needs.

⁷⁰ *T'ai-p'ing kuang-chi*, 158, p. 1135.

⁷¹ Ou-yang Hsiu, *Hsin Wu-tai shih* (Chung-hua shu-chü edn.), 17, pp. 179–80.

⁷² See Chu Hsi, *Chu Tzu yü-lei* (Chung-hua shu-chü edn.), 89, pp. 2286–87.

slower destruction by ants. In addition, newcomers to any part of the country would not have had ancestral graveyards, making changes in their customary practices somewhat easier.

CONFUCIAN OBJECTIONS TO CREMATION began to appear almost as early as the practice itself. During the Sung period, a succession of eminent scholars and thinkers revitalized Confucianism. They not only developed philosophical theories to provide the metaphysics that ancient Confucianism had lacked, they also set themselves the task of reforming the state and society according to the moral principles found in the classics. The Confucian classics had given great weight to the proper performance of rituals, and neo-Confucian scholars believed that, to combat the influence of Buddhism, it was necessary to rid life-cycle rituals of contamination by Buddhist practices. They also wanted to purify them of other noncanonical accretions, especially those that could be labeled vulgar, lower-class, or superstitious.⁷³

Confucians objected to cremation as cruel, a desecration of the corpse, barbaric, Buddhist, and unfilial. Chia T'ung speculated that cremation had been introduced by Buddhist monks who taught "the barbarian customs of the Western regions" and cited the classical rules that "a filial son serves the dead as he serves the living," and "parents give birth to the whole [body], and the child returns the [body] whole, which is filiality," arguing that cremation was mutilation.⁷⁴ Neo-Confucian philosopher Ch'eng I (1033–1107) also wanted people to think of cremation as a severe way of hurting a corpse. "Today if a fool or drunkard accidentally hits a person's ancestor's coffin, he will take great offense and want revenge. Yet he may personally drag his parent and toss him into the flames, finding nothing odd in it."⁷⁵ Scholar and statesman Ssu-ma Kuang noted that desecrating a stranger's corpse was a serious legal offense, and yet people were not offended by cremation.⁷⁶ Most vehemently criticized were children who cremated the body of a parent or grandparent. Ch'eng I, for instance, portrayed the customs of a certain place as especially evil because "even filial sons and loving grandsons through familiarity are comfortable" with cremation.⁷⁷

The philosopher Chu Hsi (1130–1200) considered cremation much worse than other ritual offenses. His objections were recorded as follows.

Someone asked: "When one's parent dies and in his or her final instructions orders the use of Buddhist or Taoist monks [for the funeral], what should one do?"

[Chu Hsi] answered: "This is a difficult problem."

[The interlocutor] asked: "May one use them or not?"

[Chu Hsi] answered: "There are some things the heart of a son cannot bear to do. This matter requires careful discussion."

[On another occasion] someone asked: "Suppose one's father is still alive when one's mother dies and he wants to follow ordinary custom in mourning garments, to use Buddhist

⁷³ On these efforts, see Patricia Ebrey, "Education through Ritual: Efforts to Formulate Family Rituals in the Sung Dynasty," in *Neo-Confucian Education: The Formative Stage*, Wm. Theodore de Bary and John W. Chaffee, eds. (Berkeley, Calif., 1989), which deals especially with weddings and ancestral rites.

⁷⁴ Chu Mu, *Shih-wen lei-chü*, 56: 13a.

⁷⁵ *Erh Ch'eng chi*, i-shu 3, p. 85.

⁷⁶ *Ssu-ma shih shu-i*, 7, p. 76.

⁷⁷ *Erh-Ch'eng chi*, wen-chi 11, p. 633. Compare Chiang Shao-yü, *Sung-ch'ao shih-shih lei-yüan* (Ku-chi ch'u-pan-she edn.), 413.

or Taoist monks, and to cremate ["transform through fire"] the body, what should one do? Should one simply disobey?"

[Chu Hsi] answered: "The others are all superficial matters that can be obeyed without harm. But cremation is unacceptable."

[The recorder of this conversation, Hu] Yung said: "This is because cremation destroys the body of the parents."

[Chu Hsi] said: "Discussing this matter [cremation] together with mourning garments and Buddhist [monks], shows an inability to distinguish between the important and the unimportant."⁷⁸

In such conversations, Chu Hsi acknowledged that not everything labeled "Buddhist" or "Taoist" was equally bad, but he classed cremation with those practices so pernicious that a son should oppose them even if doing so brought him into conflict with his father.

Neo-Confucian criticisms of cremation seldom make any reference to karma, rebirth, or any other Buddhist doctrine. Instead, the Buddhist aspect of cremation was condemned primarily on the grounds that both Buddhism and cremation were alien. By the second half of the Sung dynasty, when the alien Jurchen had occupied northern China, and later when the Mongols were coming to pose an even greater threat, anti-foreign sentiments seem to have become intense. In 1237, Wang Yüeh wrote: "Human beings are different from the birds and beasts; China is different from the barbarians . . . The customs of the Hu and Ch'iang [northern and western barbarians] are like those of the birds and beasts. The Buddhists, through their doctrines, have turned their backs on human ethical relations and cut themselves off from their kind.⁷⁹ Thus it is not surprising that after death they voluntarily take the body that should be valued because heaven and earth nurtured it, and quickly reduce it to smoke and ashes."⁸⁰ That cremation was alien to Chinese culture could also be argued by historical precedent. To demonstrate that voluntary cremation was not a part of Chinese culture, Huang Chen provided a long list of examples from Chinese history in which burning a corpse was described as the worst thing one could do to punish the dead or provoke one's enemies.⁸¹

Neo-Confucian critics of cremation did not emphasize its associations with poverty or try to make the educated feel it would be declassé to cremate. Nor did they make much of a distinction between cases in which ashes were scattered, leaving no grave, and ones in which they were buried. Neo-Confucians do not seem to have worried that, when ashes were scattered, the soul was inadequately settled or that, without a grave, ancestral offerings might not be offered. In their view, it was enough to label cremation a desecration of the corpse and thus an act of outrageous immorality.

STATE EFFORTS TO SUPPRESS CREMATION were undoubtedly motivated in large part by the sentiments Confucians expressed. The prohibition of 962 was issued on the grounds that cremation was normally harmful. In the Sung code, the punishment for cremation was stipulated as strangulation, with exceptions granted for Buddhist

⁷⁸ *Chu Tzu yü-lei*, 89, p. 2281.

⁷⁹ This is a reference to Buddhist monks "leaving the family" and not marrying.

⁸⁰ Lu Chen, *Ch'in-ch'uan chih*, 1: 26a-b.

⁸¹ *Huang-shih jih-ch'ao*, 70: 15a-17b. Compare Hung Mai, *Jung-chai sui-pi*, 13, p. 374.

monks, foreigners, and those who needed to return remains for burial.⁸² Ch'eng I complained that the law itself was not firm enough in its opposition to cremation because it allowed soldiers who died on military campaigns to be cremated and specified that cremations could not be performed less than three *li* (about one mile) away from a city.⁸³

The Chinese government, like many others, used the law to define the limits of acceptable behavior, and it often decreed punishments for actions it could hardly expect to monitor. Playing music at funerals, for instance, was declared illegal.⁸⁴ Most officials seem to have viewed the ban on cremation as one of these unenforceable statutes. Wang An-shih (1021–1086) complained that, despite the obvious evils of cremation, it was nearly impossible to prohibit a practice so well entrenched.⁸⁵

A few eleventh-century officials, convinced that they could reform society by starting at the local level, tried to eliminate cremation by educating the people and providing them with alternatives. Ou-yang Hsiu (1007–1072) had argued in his famous essay, "On Fundamentals," that the successful revival of Confucianism required curbing the influence of Buddhism in the common people's ritual activities. "If you shout at people, 'You are forbidden to follow the Buddhists and must perform our rituals and proprieties,' they will object and go away. It would be better to take a gradual approach, attracting them without their noticing."⁸⁶ The leading proponent of neo-Confucianism, Ch'eng Hao (1032–1085), was reportedly able while magistrate of Chin-ch'eng to persuade the local people to give up cremation, but they reverted to the practice when one of his successors had his own mother cremated.⁸⁷ Han Ch'i (1008–1075), later a leading statesman, tried to eradicate cremation by addressing the difficulty the poor had in arranging burials; while serving as a magistrate, he used public funds to buy land for a free graveyard.⁸⁸ Fan Ch'un-jen (1027–1101) arranged for the burial of three thousand boxes of unclaimed ashes while serving as prefect of T'ai-yüan (Shansi).⁸⁹ These local initiatives eventually led to new policies at the national level. In 1079, Ch'en Hsiang proposed having all local governments set up charitable graveyards, called "generosity gardens" (*lou-tse yüan*).⁹⁰ The court consented and issued detailed regulations on the management of the graveyards and the records to be kept concerning the location and occupant of each grave.⁹¹ Although the response of the local governments was slow, some graveyards were set up in response to these laws.⁹² Archaeological evidence shows large-scale burial of the urns of bone

⁸² Li T'ao, *Hsü tzu-chih t'ung-chien ch'ang-pien* (Chung-hua shu-chü edn.), 3, p. 65; Tou I, *Sung hsing-t'ung* (Taipei, Wen-hai reprint of 1918 edn.), 18: 9a.

⁸³ *Erh Ch'eng chi*, i-shu 3, p. 58. One can point to other inconsistencies as well. The government periodically instructed local governments to see to the burying of abandoned coffins. Sometimes, the local officials had bodies cremated or allowed cremation as an alternative to burial (Su Shih, [*Chia-t'ai*] *Kuei-chi chih* [Sung-Yüan ti-fang chih ts'ung-shu edn.], 13: 38b–39a; Hung Mai, *I-chien chih*, chia 11, p. 96). Sometimes, even members of the imperial family were cremated, at least if they died as infants or concubines (Chang Fang-p'ing, *Le-ch'üan chi*, 38: 30a–34b; Wang Hung-t'ao, "Ch'üan-chou, Nan-an fa-hsien Sung-tai huo-tsang mu," *Wen wu*, 1975.3: 77–78).

⁸⁴ *Sung hsing-t'ung*, 1: 11a.

⁸⁵ *Wang Lin-ch'uan chi* (Shih-chieh shu-chü edn.), 70, p. 441.

⁸⁶ *Ou-yang Hsiu ch'üan chi* (Shih-chieh shu-chü edn.), 17, p. 123.

⁸⁷ *Erh Ch'eng chi*, wen-chi 11, p. 633.

⁸⁸ *Han Wei-kung wen-chi*, 13, p. 202.

⁸⁹ *Sung shih*, 314, p. 10289.

⁹⁰ Hsü Tu, *Ch'üeh-sao pien* (Ts'ung-shu chi-ch'eng edn.), 3, pp. 207–08.

⁹¹ Hsü Sung, *Sung hui-yao chi-pen* (Shih-chieh shu-chü reprint), shih-huo 60: 3a.

⁹² Lo Chün and Fang Wan-li, [*Pao-ch'ing*] *Ssu-ming chih* (Sung-Yüan ti-fang chih ts'ung-chih edn.), 14: 7b, 16: 11a, 18: 11b, 20: 7a, 21: 10a; Su Shih, *Kuei-chi chih*, 13: 36b–37a; Chang Hsüan, [*Chih-cheng*]

remnants left at temples in response to these edicts.⁹³ Yet, in 1158, an official requested exemption from the prohibition on cremation for any poor person living in an area still lacking a public graveyard, which indicates that such sites were fairly common but not universally available.⁹⁴

Free graveyards were established primarily to solve the problem of the hundreds of coffins left at temples for decades or longer. The original edict ordered the burial of any coffin unclaimed for over twenty years or whose occupant was unknown, as well as bodies or bones found exposed.⁹⁵ These graveyards were also to provide a place for beggars and others who could not afford burial. The Buddhist novices who managed the graveyards were to be rewarded with ordination certificates, depending on the length of time served or numbers of people buried.⁹⁶ Even when so committed a neo-Confucian as Chen Te-hsiu (1178–1235) set up such graveyards, he had Buddhist and Taoist establishments manage them.⁹⁷ Free graveyards, in the end, did not remove all incentives to cremation. In the late eleventh century, one local official reported that better-off families accustomed to cremation did not want to be buried among inferior people in a public graveyard and so did not change their old ways; in the mid-thirteenth century, another local official similarly complained that the local population did not want their bodies lying beside the prisoners and strangers buried in the charitable graveyard.⁹⁸

In the late Sung period, Huang Chen, as magistrate of Wu county (Soochow), took another approach to eradicating cremation: he focused on the crematory, whose reconstruction he wished to forbid.⁹⁹ Despite the obvious advantages of his approach, it does not seem to have been widely copied. The Sung state regulated the ordination of monks and granted tax exemptions to temples, yet it did not use these regulatory powers to induce monks or temples to stop performing cremations. This failure indicates the limitations of the Sung state's commitment to suppressing cremation.

Alternatives to cremation could also be provided by private groups. In many of the areas where cremation was common, such as Chekiang, Kiangsu, and Fukien, descent groups were also developing in Sung times. Many were groups that simply met once or twice a year to make offerings at their common ancestors' graves. By late Sung times, the most developed were writing genealogies, erecting halls for their ancestral worship, and setting aside lands to endow ancestral rites.¹⁰⁰ A major avowed purpose of these groups was to tend graves. A secondary but still important purpose was to aid the living members of the group, especially in such ritual responsibilities as the burial of their parents. Whereas charitable graveyards set up

Chin-ling hsin-chih (Sung-Yüan ti-fang chih ts'ung-shu edn.), 2, p. 1990; Chou Ying-ho, *Chien-k'ang chih* (Sung-Yüan ti-fang chih ts'ung-shu edn.), 43: 44a–49b.

⁹³ See "Ssu-ch'uan kuan-ch'ü-nien T'ang-Sung-Ming mu ch'ing-li chien-pao," *K'ao-ku t'ung-hsün*, 1956.5: 37, for a description of the discovery of many urns with bricks marked with inscriptions like "Bones deposited at Yen-pao temple in the fifth month of Hsi-ning (1075), of unknown name, buried on the seventh day of the twelfth month of Ch'un-ning (1104), number 38." See also Ho Cheng-huang, "Sung wu-ming shih mu-ch'uan," *Wen wu* 1966.1: 53–54; "Ho-nan nan-yang fa-hsien Sung-mu," *K'ao-ku*, 1966.1: 54; and Sung Ts'ai-i and Yü Sung, "T'an Ho-nan Nan-hua hsien fa-hsien Pei-Sung ti lou-tse yüan," *Ho-nan ta-hsüeh hsüeh-pao*, 1986.4: 53–58.

⁹⁴ *Sung-shih*, 125, pp. 2918–19.

⁹⁵ Su Shih, *Kuei-chi chih*, 13: 36b.

⁹⁶ *Sung hui-yao chi-pen*, shih-huo 60: 3a, 7b–8a, 9b.

⁹⁷ Chang Hsüan, *Chin-ling hsin-chih*, 12: 71b.

⁹⁸ Chang Li-hsiang, *Sang-tsang tsa-lu*, 1b; Lu Chen, *Ch'in-ch'uan chih*, 1: 24a–28a.

⁹⁹ *Huang-shih jih-ch'ao*, 70: 14b–18a.

¹⁰⁰ Ebrey, "Early Stages in the Development of Descent Groups."

by the local government carried a social stigma, assistance from kin was entirely appropriate in the Chinese tradition, especially in funeral matters, which were in theory the responsibility not merely of the deceased's children but of all "mourning relatives," including some as distant as third cousins. In the fourteenth century, neo-Confucian teacher Wu Hai wrote rules for his descent group that included prohibitions against becoming Buddhist monks or Taoist priests, holding Buddhist funeral services, and practicing the "barbarian ritual" of cremation.¹⁰¹

Efforts by officials and private citizens to discourage cremation may have checked its growth, but neither archaeological nor literary evidence suggests that the practice of cremation declined in late Sung or Yüan times.¹⁰²

DURING MING AND CH'ING TIMES, the practice of cremation by Han Chinese did decline. Chu Yüan-chang (1328–1398), the founder of the Ming dynasty (r. 1368–1398), was by no means a narrow Confucian or even an opponent of Buddhism.¹⁰³ Nevertheless, he took a strong stand against cremation. According to one account, Chu stood on the Nanking city wall during the civil war and was repelled by the odor of cremations taking place. His advisor T'ao An (1312?–1368) took advantage of this opportunity to convince him that cremation was inhumane. In ancient times, he argued, people even buried old bones found exposed, but "the current age has been perverted by the customs of the Hu [northern barbarians] so that some burn [the body] and toss the ashes into water. How can a filial son or loving grandson bear this in his heart? Nothing injures reciprocal obligation or destroys customs as much as this." Swayed by these arguments, Chu Yüan-chang ordered that his army begin to bury bodies rather than burn them.¹⁰⁴ Later, in 1370, he issued a general prohibition against cremation. There were not even to be exceptions for officials who died away from home; the local government was required to supply the funds to have their bodies returned home. When the emperor repeated this prohibition in 1372, he complained about the persistence of the customs of the "evil" Yüan dynasty, of which the worst was cremation.¹⁰⁵ And the Ming code, issued in 1397, explicitly stated that, even if, before dying, a family elder had asked to be cremated, the act was illegal and punishable (though ordinary people who died away from home could be cremated). The penalty for cremating a senior relative was decapitation and for cremating a child or grandchild, a beating of eighty strokes.¹⁰⁶ Not only were these laws stricter than Sung or Yüan ones, the early Ming government's efforts at social control were more pervasive than those of earlier governments, and therefore these laws were probably more

¹⁰¹ *Wen-kuo chai-lu* (Ts'ung-shu chi-ch'eng edn.), 1, pp. 1–3.

¹⁰² Compare the views of James Watson, who argues that the state played a major role in achieving a high level of uniformity in Chinese funerary rites but largely ignored rites of disposal (burial, cremation, exhuming bodies for reburial of the bones, and so on); "The Structure of Chinese Funerary Rites: Elementary Forms, Ritual Sequence, and the Primacy of Performance," in Watson and Rawski, *Death Ritual in Late Imperial and Modern China*, 15–19.

¹⁰³ John D. Langlois, Jr., and Sun K'o-k'uan, "Three Teachings Syncretism and the Thought of Ming T'ai-tsu," *Harvard Journal of Asiatic Studies*, 43 (1983): 97–139. See also John W. Dardess, *Confucianism and Autocracy* (Berkeley, Calif., 1983), for Chu Yüan-chang's relationships with Confucian advisors.

¹⁰⁴ Huang Yü, *Shuang huai sui-ch'ao* (Ts'ung-shu chi-ch'eng edn.), 1, p. 12.

¹⁰⁵ Yao Kuang-hsiao, et al., *Ming shih-lu* (Academia Sinica reprint edn.), T'ai-tsu, 53: 10b–11a; *Ming shih* (Chung-hua shu-chü edn.), 60, p. 1492.

¹⁰⁶ Kao Chü, *Ta Ming lü chi-chieh fu-li* (Ming-tai shih-chi hui-k'an edn.), 12: 25a (p. 989).

rigorously enforced than were earlier ones.¹⁰⁷

Archaeological evidence suggests that cremation declined very rapidly in the early Ming.¹⁰⁸ Nevertheless, cremation did not disappear entirely, as complaints of its practice continued to be made from time to time.¹⁰⁹ Unlike complaints voiced in the Sung, these Ming and Ch'ing ones rarely suggest that the elite practiced cremation even in special circumstances or that poor people cremated their parents. Instead, cremation seems more and more to have been confined to special situations or the unimportant dead. Chang Li-hsiang (1611–1674) reported that, in his home area, cremation was particularly common for children.¹¹⁰ In the mid-nineteenth century, Justus Doolittle reported that dead lepers were burned, a measure thought to prevent spread of the disease.¹¹¹ J. J. M. de Groot, who spent many years in China in the late nineteenth century, wrote that he had never witnessed or even heard rumors of cremation except for Buddhist monks and nuns and soldiers who died away from home.¹¹²

In what sense, if any, can the decline of cremation be seen as a victory of neo-Confucianism? Leading neo-Confucian teachers—Ssu-ma Kuang, Ch'eng I, and Chu Hsi—opposed cremation, and their objections became better known in Ming times. Ssu-ma Kuang's denunciation of cremation was included in Chu Hsi's *Family Rituals* [*Chia-li*], which circulated extensively from the early thirteenth century on.¹¹³ The *Family Rituals* was copied into the *Hsing-li ta-ch'üan* [Comprehensive collection on nature and principle], an imperial-sponsored compendium of neo-Confucian writings published in 1415 and widely read by examination candidates in the Ming. Thus, by Ming times, the disrepute of cremation must have been

¹⁰⁷ See Edward L. Farmer, "Social Regulations of the First Ming Emperor: Orthodoxy as a Function of Authority," in K. C. Liu, ed., *Orthodoxy in Later Imperial China* (Berkeley, Calif., 1990). On the severity of the first Ming emperor's enforcement of laws, see also Dardess, *Confucianism and Autocracy*, 183–253.

¹⁰⁸ In areas inhabited predominantly by Han Chinese, over 90 percent of the excavated cremation burials date from the tenth through fourteenth centuries (Hsü P'ing-fang, personal communication). In non-Han areas, or in non-Han graves in mixed areas, cremation burials persisted for several more centuries.

¹⁰⁹ Wu K'uan (1436–1504) complained that, because of the population density in Soochow, the lower classes (*hsiao-min*) burned the bodies of their parents. In 1497, the prefect banned the practice, and three years later he established a charitable graveyard to help those who as a consequence had left bodies unburied. Still, near the end of the Ming, another scholar observed that, in this same city, while the educated performed proper funerals, "brokers in the markets and government servants all use cremation"; *Wu-hsien chih* (Chung-kuo fang-chih ts'ung-shu reprint of 1933 edn.), 41: 28a. Chang Hsüan (1558–1641) quoted a proclamation issued by Teng Huai-ch'ang, prefect of Wen-chou on the coast of Chekiang, decrying the prevalence of cremation among the poor in spite of the first Ming emperor's prohibition of it. Poverty was no excuse for cremation, he insisted, as there was a charitable graveyard in the area. Any further cremations, he declared, would be punished according to the law; *Hsi-yuan wen-chien lu* (Chung-hua wen-shih ts'ung-shu reprint), 4: 39b–40a. In the early Ch'ing, Chang Li-hsiang (1611–1674), from T'ung-hsiang (Chekiang), complained of the persistence of cremation in his home area, comparing the Yüeh region (roughly Fukien), where burial was universal, to his area, where cremation flourished; *Sang-chi tsa-shuo* (in *Tu-li ts'ung-ch'ao*), 5a–b. Chang Tz'u-ch'en mentioned the case of a poor man in Hangchow who paid the modest sum of fifty cash to have his mother cremated; cited in Wang Fu-li, *Chia-li pien-ting* (1707 edn.), 7: 52b. Ku Yen-wu (1615–1682) mentioned that cremation had flourished in the Chiang-nan area (roughly Chekiang and Kiangsu) since the Sung dynasty, quoting in full two Sung memorials against the practice but no later evidence. Commenting on this passage over a century later, Huang Ju-ch'eng (1799–1837) noted that cremation was still practiced in Hangchow in his day (though apparently not throughout the whole area); *Jih-chih lu* (Shih-chieh shu-chü edn.), 15, pp. 366–68.

¹¹⁰ *Sang-chi tsa-shuo*, 5a–b. See similar comments in Wang Fu-li, *Chia-li pien-ting*, 7: 53a–b.

¹¹¹ Justus Doolittle, *Social Life of the Chinese*, 2 vols. (Ch'eng-wen reprint of 1865 edn.), 2: 257.

¹¹² De Groot, *Religious System of China*, 3: 1415. For the use of cremation by Buddhist monks and nuns in late nineteenth and early twentieth-century China, see W. Percival Yetts, "Notes on the Disposal of Buddhist Dead in China," *Journal of the Royal Asiatic Society*, n.s., 43 (1911): 699–725.

¹¹³ On this text, see Ebrey, "Education through Ritual."

well known to the educated and probably had an effect on their choices of mortuary practices, even when they had to deal with death away from home and death of servants or other less important people in their households. Their behavior in turn may have influenced the practices of people of lower social standing, especially those aspiring to literati status. Confucian propaganda would have had its greatest impact on how people disposed of their parents, since this act was highly public and commonly used as a gauge of respectability.

Increased awareness of the neo-Confucian objections to cremation nevertheless does not wholly explain the decline in cremation. In these same books, Ssu-ma Kuang, Ch'eng I, and Chu Hsi give powerful arguments against the performance of Buddhist services for the dead and against delayed burial, yet there is plenty of evidence that even literati and officials in Ming and Ch'ing times still asked Buddhist monks to perform the ceremony, and delayed burial remained endemic.

Stricter laws against cremation, more vigorously enforced, undoubtedly discouraged its practices, but they were as much a result of emperor Chu Yüan-chang's intense distaste for practices associated with the alien Yüan dynasty as of his acceptance of neo-Confucian doctrines. The enduring perception that cremation was a foreign or barbarian custom provides the best explanation for why people in the early Ming dynasty were willing to give it up. Other Buddhist practices quickly lost all overtones of foreignness. Burning incense when praying to ancestors, for instance, did not seem in Sung or Ming times to be a foreign custom. Nor did services aimed at ensuring a better rebirth. But cremation never ceased to be alien. The handling of dead bodies admittedly arouses stronger emotions than offerings of prayers. Even more important, all through this period, cremation continued to be practiced by non-Han peoples living adjacent to Han-settled areas. The alien character of cremation was underlined in the Sung and Yüan law codes, in which cremation was permitted for foreigners. With the massive repudiation of everything associated with the Mongols in the early Ming, cremation was no longer simply a convenient, though inferior, method of disposing of the dead, introduced by Buddhists, but a foreign custom that had flourished under the Mongols.

The alien overtones of cremation probably influenced common people as much as Chu Yüan-chang, who was himself of peasant origin. There is no evidence that cremation was suppressed by force, by large-scale punishment of offenders or destruction of crematoria. Many poor people, resentful of the Mongols, apparently decided that they would not cremate their parents even though their grandparents had been cremated, because the feelings of revulsion at the thought of treating their bodies in a "barbarian" way outweighed its convenience. Since cremation had never been widely seen as an end in itself, people could turn away from it relatively easily.

BY WAY OF CONCLUSION, I would like to compare the historical processes involved in these two changes in customs, the increase in the use of cremation and its subsequent decline. In each case, ideas of different origins prepared people to change their attitudes toward the dead body. Some of the ideas that promoted cremation came from Buddhist doctrine—such as ideas about rebirth, some from folk Buddhism—such as ideas about relics, and some from indigenous culture with, at best, weak ties to Buddhism—such as ideas about bones and graves. The decline of cremation was influenced by anti-foreign feelings, which greatly enhanced the

Confucian view that the old ways of treating the dead were the morally correct ones.

In both cases, centuries passed between the introduction of new ideas and changes in behavior. Customs did not change until new ideas had fostered new institutions and dislocations jarred people out of routines. The most important institutional development in supporting cremation was the entry of Buddhist temples and monasteries into the funeral business: providing funeral services, storing coffins, and above all operating crematoria open to the public. The dislocating events of greatest significance were the rebellions, invasions, dynastic wars, and migrations of the late ninth and tenth centuries. In the case of the decline of cremation, the state provided the most important institutional framework, through both its laws against the practice and its organized social welfare measures. The significant dislocating event was the reestablishment of a native Chinese dynasty and a general repudiation of Mongol ways.

The processes that led to the acceptance of cremation and those that led to its rejection were, however, not entirely alike. Above all, coercion or threats played no part in the increase in cremation. Buddhist lay people were not ordered to cremate or warned of any dire consequences to themselves or their dead loved ones if they failed to cremate. They chose cremation on their own, imitating monks but also finding it convenient, economical, and in some cases, perhaps, emotionally satisfying. The decline of cremation may well have been perceived as voluntary, people losing interest in cremation for a wide range of reasons, from revulsion for the practice to desire to imitate Confucian norms. But the coercive role played by the elite and the state is undeniable. Neo-Confucian scholars labeled as unfilial those who cremated; the state declared them criminals and put mechanisms in place to punish them.

Review Article
The Attack on "Historicism":
Allan Bloom's Indictment of Contemporary
American Historical Scholarship

FRED MATTHEWS

ALLAN BLOOM, the University of Chicago political scientist, has attracted extensive attention with the most highbrow best seller of the 1980s, his attack on the contemporary university, *The Closing of the American Mind*.¹ Historians have a major stake in the appraisal of Bloom's work, since it directly challenges the assumptions (often unconscious) with which we interpret our data. It is easy to overlook this challenge, since Bloom dismisses professional history with a brief, if pointed, attack on the work of Charles A. Beard and Carl Becker. But the rejection of these professional elders is closely tied to his indictment of related thinkers—John Dewey, Oliver Wendell Holmes, Jr., and Margaret Mead—and to his extended assault on the "historicism" of Friedrich Nietzsche, which, Bloom argues, was introduced through the cult of Sigmund Freud and Max Weber in the 1940s and 1950s, and has more recently weakened the ability of Americans to engage in a searching, vigorous quest for truth. These thinkers share axioms about reality and its fruitful interpretation with the majority of professional historians, whether or not we are aware of it. And Bloom sees Beard and Becker, with some justice, as leading the first stage of retreat from the traditional belief in trans-historic, trans-cultural absolutes on which Western civilization has rested.²

Closing is a rich, often brilliant, and disturbing book, which speaks powerfully to a broadly shared fear that standards, discipline, and the ability to engage in dialogue about controversial issues are declining or disappearing in the contemporary educational system. Many of its probes into popular culture (which is seen as a cause of decline parallel to, and sanctioned by, the loss of philosophic unity) are both amusing and perceptive. Bloom finds emblematic significance, for instance, in the meeting of Ronald Reagan and Michael Jackson, the plastic men of widely separated generations. Bloom's concern for the educational consumers—the students—in a system run more and more along Stakhanovite norms of scholarly productivity, may evoke sympathy from those who dispute his diagnosis. But many aspects of his analysis are disturbing: the intensity of his condemnation of the academy, the nature of his preferred alternative, and his eagerness to impeach enemies on personal grounds. For example, Bloom refers to Margaret Mead,

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¹ Allan Bloom, *The Closing of the American Mind* (New York, 1987).

² Bloom, *Closing*, 147–48.

presented as representative of modern anthropology, as one of the "sexual adventurers . . . who found America too narrow."³ It is one of the paradoxes of Bloom's work that he himself employs the technique of "unmasking," or muckraking contextualism, which he rejects when used by his critics. This style of attack, common in much of contemporary journalism and political polemic, raises disturbing, if vicariously enjoyable, vistas of a future form of scholarly debate in which gossip is a legitimate part of appraisal. Indeed, this may be an emerging issue for the 1990s: Peter Novick's splendid *That Noble Dream* observes that the perspectivism and contextualism of the New History may undercut the interdict on *ad hominem* arguments, for they suggest that the grounds on which scholars develop interpretations include a variety of personal concerns as well as a rigorous analysis of data.⁴

ACCORDING TO ALLAN BLOOM, academics, in turning away from timeless categories of interpretation, have robbed their students of the myth of objective truth necessary for effective reasoning. Combined with this pedagogical concern is a patriotic one: that the academy's value-subversive "historicism" erodes students' faith in the founding myths of the United States. The implicit premise of Bloom's attacks on academic relativists is not absurd: a modern, "contrived" polity like the United States, lacking deep Burkean roots in shared experience, has a special need for a consciously inculcated myth of the virtuous founding, for axiomatic principles exempt from historical criticism. This myth had once been dominant; as Daniel Boorstin explained in 1953, the American political tradition since the founding revolved around a faith that "the earliest settlers or Founding Fathers equipped our nation at birth with a perfect and complete political theory, adequate to all our future needs."⁵ Central to Bloom's argument is the tenet that the Framers of the Constitution believed in absolutes, in natural rights based on universally shared human traits. Therefore, challenges to belief in these absolute principles threaten national morale and unity. The *trahison des clercs* came in the early twentieth century, with what Morton White dubbed the "revolt against formalism"—a rebellion against the kind of traditional scholarship, drawing on biblical criticism, that assumed that documents have one original meaning, that we discover truth by recovering this meaning and applying it to contemporary events. Its major champions were the pragmatic philosophers William James and John Dewey; the historians Beard and Becker; the legal thinkers Oliver Wendell Holmes, Jr., and Louis Brandeis; the economists Thorstein Veblen and Wesley Clair Mitchell; the anthropologists Franz Boas, Ruth Benedict, and Margaret Mead. Although many of the specific ideas of these antiformalists were subsequently rejected (some were revived in the 1960s), Bloom is right on a broad level of generalization to assert that much of twentieth-century historiography and social science has been "modern" in assuming that change and process are the rule; that documents derive meaning from the horizontal, historical context that generated them—not from a search for the ultimate abstract meaning of their words.⁶

³ Bloom, *Closing*, 76 (Reagan); 33 (Mead).

⁴ Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (New York, 1988), 219–22.

⁵ Daniel J. Boorstin, *The Genius of American Politics* (Chicago, 1953), 8–10.

⁶ Morton White, *Social Thought in America: The Revolt against Formalism* (1949; Boston, 1957). For echoes of this point by Albert Shanker and other officials of the American Federation of Teachers, see Katy Butler, "Teach Democracy, U.S. Schools Urged," *San Francisco Chronicle*, July 5, 1988, A2. The

This shift in interpretive style, from hermeneutic and deductive to what Bloom calls "historicist" and Polonius might have called "empirical-contextual-historical," helped to undermine educated belief in the timelessness of the values long considered a basic part of the documents of the "founding of the United States." Enter Beard and Becker, the leading historical antiformalists. Bloom writes:

The American tradition is old and established, and its meaning is articulated in simple, rational speech that is immediately comprehensible and powerfully persuasive to all normal human beings . . . Our heroes and the language of the Declaration contribute to a national reverence for our Constitution . . . But the unity, grandeur, and attendant folklore of the founding heritage was attacked from so many directions in the last half century that it gradually disappeared from daily life and from textbooks . . . [This debunking of national myths gradually spread from the "higher intellectual circles" out to the schools.] The leading ideas of the Declaration began to be understood as eighteenth-century myths or ideologies. Historicism, in Carl Becker's version (*The Declaration of Independence: A Study in the History of Political Ideas*, 1922) both cast doubt on the truth of the natural rights teaching and optimistically promised that it [historicism] would provide a substitute. Similarly Dewey's pragmatism—the method of science as the method of democracy, individual growth without limits, especially natural limits—saw the past as radically imperfect and regarded our history as irrelevant . . . Then there was the Marxist debunking of the Charles Beard variety, trying to demonstrate that there was no public spirit, only private concern for property, in the Founding Fathers, thus weakening our conviction of the truth or superiority of American principles or our heroes.

Thus openness has driven out the local deities, leaving only the speechless, meaningless country. There is no immediate, sensual experience of the nation's meaning or its project, which would provide the basis for adult reflection on regimes and statesmanship.⁷

The last sentence is crucial for understanding Bloom, who edited Rousseau's *Emile* and envisions an intensely focused, womb-like political community nurturing its members. Ironically, one is reminded of Ruth Benedict's vision of the nurturing function of a culture for those who share it. But Bloom insists on a single, "given" political culture for the entire society, one that imposes itself actively through education on the minds of new members, who then think unquestioningly from its axioms. At a later point, he praises the coercive, paradigm-closing program of Rousseau's *Social Contract*. The community, or "people," whose unanimity is necessary to form a political "regime," "will not automatically result from individual men's enlightenment about their self-interest. A political deed is necessary." As Rousseau himself phrased it, each individual must be transformed "into a part of a greater whole from which that individual . . . gets his life and his being."⁸ Bloom rightly praises Rousseau's "harshness" in frankly stating the degree of coercion needed to create the kind of "community" that generations of intellectuals have hoped to find emerging spontaneously from natural human cooperation, either traditional or intentional. Still, one of the major ambiguities of Bloom's program emerges with his use of Rousseau. He believes that an effective political culture will allow citizens to reason about "regimes and statesmanship." Yet the degree of compulsory socialization needed to create a political community suggests that only

abandonment in the 1960s of didactic teaching of the superiority of American politics was singled out as a major cause of current discontent. On the problems raised by the historicist-contextualist interpretation of thinkers who believe in absolute values, see John Diggins, "The Oyster and the Pearl: The Problem of Contextualism in Intellectual History," *History and Theory*, 23 (1984): 151–69.

⁷ Bloom, *Closing*, 56.

⁸ Bloom, *Closing*, 189.

a narrow range of discussion of means toward axiomatic ends would be allowable, or even possible, for the majority of citizens, whatever level of sophistication was achieved by philosophers and their chosen pupils.⁹

Here we can see the paradox in Bloom's title: the "closing" he laments stems precisely from the "openness" to the variety of experience championed by Jamesian pragmatism. The mythical mind is "closed" by its indiscriminate tolerance, its reluctance to reason toward the one correct answer that will support binding value judgments. In this context, Justice Holmes's "clear and present danger" standard for free speech was a dangerous opening, because it was a vague and flexible—that is, contextual—standard rather than a clear, precise, and eternal standard, which left nothing up to the judge applying it. Bloom describes the process of decline:

But openness . . . eventually won out over natural rights, partly through a theoretical critique, partly because of a political rebellion against nature's last constraints. Civic education turned away from concentrating on the Founding to concentrating on openness based on history and social science. There was even a general tendency to debunk the Founding to prove the beginnings were flawed in order to license a greater openness to the new. What began in Charles Beard's Marxism and Carl Becker's historicism became routine. We are used to hearing the Founders charged with being racists, murderers of Indians, representatives of class interests.¹⁰

This critique rests on a strong set of assumptions at odds with those of its targets. Bloom is a disciple of Leo Strauss, one of the great German emigrés who, in the 1940s and 1950s, extended the intellectual range of American universities. Strauss taught at the University of Chicago, and, together with the deductive laissez-faire economists Frederick Hayek, Leopold von Mises, and Milton Friedman, and the neo-Thomist, secularized Catholic philosophers Robert Hutchins and Mortimer Adler, he made Chicago the leading center of absolutist, antipragmatic, and anti-historicist thinking. (Former Judge Robert Bork was a student and professor at Chicago, and his legal doctrines show the influence of Friedman's economics and Strauss's theory of textual analysis.)¹¹

As a hermeneutic method, "Straussianism" is narrow and intense, focusing on internal analysis of documents, seeking to understand "original meanings"—which may or may not coincide with their writers' intentions—by using a "structural"

⁹ The Scottish philosopher, Alasdair MacIntyre, shares with Bloom the belief that living morality derives from a primary community of interpretation; see MacIntyre, *Whose Justice? Which Rationality?* (London, 1988); and the searching critique by Thomas Nagel, "Agreeing in Principle," *Times Literary Supplement*, 4449 (July 8, 1988): 747–48.

¹⁰ Bloom, *Closing*, 28–29.

¹¹ Bloom's attack on "historicism" can be better understood if read with Leo Strauss's categorical distinction between history and political philosophy as separate disciplines: the former concerned with origins and influences, the latter with reasoning about concepts in a timeless present; Leo Strauss, "Political Philosophy and History," in *What Is Political Philosophy? and Other Studies* (Glencoe, Ill., 1959), 56–77. A convenient summary of the Straussian approach to political philosophy is by Nathan Tarcov and Thomas L. Pangle, "Leo Strauss and the History of Political Philosophy," in Leo Strauss and Joseph Cropsey, eds., *History of Political Philosophy*, 3d edn. (Chicago, 1987), 907–34. A discussion of the Straussian movement with special reference to its treatment of the Constitution is by Gordon Wood, "The Fundamentalists and the Constitution," *New York Review of Books*, 35 (February 18, 1988): 33–40. Also essential is Sidney Hook's comprehensive appraisal, "The Closing of the American Mind: An Intellectual Best-seller Revisited," *American Scholar*, 58 (Winter 1989): 123–35. On the earlier revival of Thomist thought at Chicago, see Edward A. Purcell, Jr., *The Crisis of Democratic Theory: Scientific Naturalism and the Problem of Value* (Lexington, Ky., 1973). The founding text of Straussianism is Leo Strauss, *Natural Right and History* (Chicago, 1953). On Judge Bork, and the similarities between his jurisprudence and the Straussian movement, see Jacob Weisberg, "The Cult of Leo Strauss: An Obscure Philosopher's Washington Disciples," *Newsweek*, 110 (August 3, 1987): 61; and Renata Adler, "Coups at the Court," *New Republic*, 197 (September 14, 1987): 37–48.

analysis that stresses the relation of words and sentences to one another. Straussianism is one of several "originalist" schools. While they lack precise consensus on the limits of the relevant context of interpretation, all agree on the importance of the key document itself and a small penumbra of other documents whose connection is clear. For more narrow "originalists," the records of the ratifying conventions for the Constitution, and of the debates on amendments, are relevant to its intended meaning but only in their specific assertions—Robert Bork, for instance, wishes to rule out any general understanding of rights based on journalism and private letters. A different "originalist" view is presented by Harry Jaffa, in my judgment the greatest of the Straussian writers on history, who insists on a broad reading to discover the kind of consensus that underlies the specific constitutional texts.¹² From Jaffa's and Bork's criterion of relevance, Charles Beard was a misleading debunker, because any narrow, self-interested motives, whether accurately imputed or not, were simply irrelevant; it is the conscious, calculated general statements that determine meaning. And, within limits, there is force in this argument: to reduce complex, theoretical structures to the private motives of a few people is like explaining the symphonies of Beethoven as the result of personal drives, conscious or unconscious. Beard did, in his early work, appeal to the cynical sophomore in each of us, eager to discredit opponents by "unmasking" their motives rather than wrestling in depth with their arguments.¹³

TO BEGIN TO APPRAISE THE VALIDITY of Bloom's indictment of historians, we need to raise two questions. Most narrowly, is there substance in the view of Charles Beard as a partisan who attempted to discredit vital American myths through tendentious misreadings of events and documents? Are the historical and ethical visions of Beard and Bloom as sharply opposed as Bloom's attack would suggest? To take the most specific issue first: Beard's reading of *The Federalist* was colored by his eagerness to find canonical authority for his own reading of the Constitution as an "economic document." We need therefore to discover what he meant by an economic document and to determine how accurate is his reading of *The Federalist*. In his 1913 book, Beard gives three major reasons for insisting that the Constitution was aimed at protecting "economic" or "material" interests. The massive press and pamphlet literature surrounding the issue showed that the evils to be remedied were primarily economic; "the records of the Philadelphia convention reveal the successive steps in the building of the framework of the government under the pressure of economic interests"; and *The Federalist* itself "presents an economic interpretation . . . by the men best fitted, through an intimate knowledge of the ideals of the framers, to expound the political science of the new government." Indeed, Beard maintained that *The Federalist* was "the finest study in the

¹² On the way in which Bork's hermeneutics rule out general statements of ethical principle, see Jamie Kolver, "Reagan's Next Justice? Robert Bork and the Constitution," *Nation*, 237 (October 1, 1983): 262–68; and the vigorous critique by Harry V. Jaffa, "Judge Bork's Mistake," *National Review*, 40 (March 4, 1988): 38–40.

¹³ Straussianism can be seen as a thoroughgoing reaction to the "debunking" or "unmasking" mode of analysis that characterizes much of modern thought. This "unmasking" mode is based on the assumption of a conflict between a hidden "reality" and the "rhetoric" with which people justify or disguise the hidden forces that mold events. A famous analysis of this mode of thought is by Lionel Trilling, "Reality in America," *The Liberal Imagination: Essays on Literature and Society* (New York, 1953), 1–19.

economic interpretation of politics which exists in any language.”¹⁴

Beard's use of the term “economic” reveals one reason why his reading of *The Federalist* (and of the Constitution) has been controversial. He has an expansive, if not boundless, definition of “economic”—he seems to mean anything that is “material” as opposed to “ideal” or purely theoretical and disinterested, in the classic sense of lacking any shred of personal advantage to its advocates. Thus Beard lists as one of the Constitution's appeals “to some material and substantial interest,” the appeal “to the people at large in the name of protection against invading armies and European coalitions.”¹⁵ This item is reasonable: defense, like internal police, serves to protect property, among other things. But, in Beard's expansive vision, almost everything can be seen as “material” and therefore “economic,” except perhaps an abstract interest in botany or the philosophy of Plato. And both botany and Plato could be incorporated into a materialist interpretation easily enough—the first as an agent of economic exploitation, the second as an agent of control over dangerous opinion, which indeed it is in the works of Allan Bloom! The question then becomes whether an interpretation that rests on imperialistic semantics—on reinterpreting everything as “ultimately material”—retains explanatory power.

Later critics of Beard, such as Douglass Adair and Gordon Wood, argued that his narrow conception of action based on motives did not allow for the existence of a larger theoretical structure that was shared by eighteenth-century thinkers. They felt that possession of property was a prerequisite for independence of judgment and therefore for civic virtue; thus to try to protect property was to provide the necessary infrastructure for stable republican government. Beard recognized the existence of this theoretical level; he expounds on it in his discussion of *The Federalist*. But the critics are largely correct: when he tries to relate theory to interest and action, he leaves the impression that theory is an umbrella of rhetoric that serves to justify the specific impulses of greed giving the system its energy. Beard has James Madison in *The Federalist* believing that “the theories of government which men entertain are emotional reactions to their property interests.” And, discussing the Constitution, Beard comments, “Of course it may be shown that the ‘general good’ is the ostensible object of any particular act; but the general good is a passive force, and unless we know who are the several individuals that benefit in its name, it has no meaning. When it is so analyzed, immediate and remote beneficiaries are discovered; and the former are usually found to have been the dynamic element in securing the legislation.” This statement shows how far Beard was from Marxism. He believed in the sort of republican intentionalism analyzed by Gordon Wood, which assumes that events are caused by the conscious intention of identifiable people with personal interests. This view is rationalist, individualist, and pre-sociological, interpreting events in terms of specific individual and group intentions.¹⁶

¹⁴ Charles A. Beard, *An Economic Interpretation of the Constitution of the United States*, 2d edn. (1913; 1935; New York, 1960), 152–53. Minor caveats arise: we might dispute whether Beard was a “Marxist,” or an antique Republican moralist, who retained the traditional belief in conscious intention as the prime cause of political events. Indeed, a fully developed Marxian reading of the Constitution might transcend some of the problems in Beard's interpretation.

¹⁵ Beard, *Economic Interpretation*, 154.

¹⁶ The quotations are from Beard, *Economic Interpretation*, 157, 155. On property as a prerequisite for independence and therefore civic virtue, see J. G. A. Pocock, *Politics, Language and Time* (London, 1973), 88–93. The most influential critique of Beard on these conceptual grounds was by Douglass Adair in the 1950s; Adair's essays are now collected in *Fame and the Founding Fathers*, Trevor Colbourn, ed. (New York, 1974), esp. 75–123. A separate line of attack concerned the accuracy of Beard's analysis of the

At this point, Beard might protest: but Alexander Hamilton, John Jay, and James Madison agreed with me! Read *The Federalist*, and you will see that it offers a theory of politics as based on conscious economic interest, just like mine. But, when we examine closely Beard's reading of *The Federalist*, we see that Madison cannot be neatly subsumed under Beard's definition of political theories as men's "emotional reactions to their property interests." Madison acknowledged that "a landed interest, a manufacturing interest, a mercantile interest, a moneyed interest, with many lesser interests, grow up of necessity in civilized nations, and divide them into different classes, actuated by different sentiments and views." He was worried that these competing interests would control government without regard for the larger public good: hence the effort to create a self-regulating, self-balancing mechanism. However, *Federalist* 10 is full of other statements suggesting that Madison believed in the possibility of people learning through education of the need to transcend immediate personal interests. Thus there is a crucial difference between Madison and Beard here: Madison has a more complicated, if less consistent, theory, one that accepts the possibility of "enlightened statesmen" and of a definable, recognizable "public interest" not necessarily identical with the self-interest of a faction that happens to be in power. Madison is "pre-modern," in Allan Bloom's usage, in preserving as an ideal the classical norm of disinterested, rational intelligence as a potential force for good. Beard, on the other hand, has taken one aspect of Madison's complex system, chopped it out of context, and universalized it into a deterministic theory of interest groups.¹⁷

Madison's writing resists such reduction and offers an interesting dualism. He saw two major types of faction, first, the economic, which is "rational" in being based on the acquisitive faculties of human beings, and is limited, in the sense that few people really want all the property in the world. The other type of faction, less frequent but more dangerous, is "irrational" in being based on passion, or sentiment, and coalesces around admiration for particular people, or groups, or ideas—like religious doctrines. This source of faction in "opinion" is more dangerous than that based on property because it is a zero-sum game: political disputes based on "opinion" usually entail the desire to either convert or liquidate the opposition. In making this distinction, Madison anticipated an idea familiar to the political science of the mid-twentieth century, the contrast between economic and "cultural" quarrels in politics. And we can see it at work now: American politics have been so bitter in the 1980s because so heavily based on a cultural contrast between the "mainstream-American" suburbs and the "rainbow coalition" of minorities that remain a force in urban politics. But this kind of conflict could have been familiar to Beard, too, since it characterized political disputation a century ago, as in the famous epithet of 1884, "Rum, Romanism, and Rebellion." Beard, for all his learning, was reluctant to grant causal status to noneconomic factors, in part because they are more diffuse and emotional and would have blurred an explana-

economic interests of the Founders (personally or as local representatives). Here the major work was Forrest McDonald, *We The People: The Economic Origins of the Constitution* (Chicago, 1958). McDonald's *Novus Ordo Seclorum: The Intellectual Origins of the Constitution* (Lawrence, Kans., 1985) is a solid summary of recent scholarship. An excellent summary, with bibliography, of the voluminous literature on the controversy is James H. Hutson, "The Constitution: An Economic Document?" in *The Framing and Ratification of the Constitution*, Leonard W. Levy and Dennis J. Mahoney, eds. (New York, 1987), 259–70.

¹⁷ Alexander Hamilton, James Madison, and John Jay, *The Federalist*, Benjamin Fletcher Wright, ed. (Cambridge, 1961), 130–33. An excellent recent study is David F. Epstein, *The Political Theory of "The Federalist"* (Chicago, 1984).

tion that needed simplicity to serve as the basis for action. In so reasoning, he anticipated a persistent "professional deformation," which helps to explain the persistent power of his work. It would be enlightening to put together a history course on a group of major conflicts and then try to classify them according to Madison's dichotomy. It is likely that a large share of a randomly selected list would illustrate the power of "opinion" but that contemporary scholars would tie themselves into knots trying to find that "ultimate" material cause.¹⁸

What, then, is the historical value of Beard's account? It is a simple question, but the answer is complex. Beard sometimes interpreted texts in a very partisan fashion, using the "flashlight" method of illuminating and stressing what supported his theory. At this level, Gordon Wood is right to say that Beard was often a poor historian. But few professional historians would claim that Beard's focus on economic interests was totally unproductive. Even if wrong on some details, Beard brought a new range of sources and areas of influence into a historical profession that in 1913 was heavily formalistic and uncritical. To shrink the scope of historical inquiry down to its pre-Beardian range would be to make it yet another agent of patriotic self-congratulation in a culture already saturated with nationalism.¹⁹

The difference between Madison and Beard on the issue of disinterestedness may help to explain why contemporary champions of reason and civic virtue, like Allan Bloom, make Beard a major villain. We receive from Beard the message that, at least in a property-based republic, the self-interest of the powerful will usually dominate. His reductive reading of Madison suggests the impossibility of a disinterested intelligence rising above personal bias. Such a position threatens university teachers, who claim special privileges—tenure and time for research—on the grounds that they are capable of transcending the special, partial interest of any of the groups or "factions" with whom we identify as human beings. If, as many now do, we universalize the early Beard's view of pervasive factionalism to embrace the academic world, professors can no longer justify this claim. Bloom's anger at such a situation reflects his fear that covens of "factional" scholars, whose theories rest on the rejection of what he sees as the universal authority of the Western classics, will make the universities collections of "ignorant armies clashing by night," closed-minded polemicists incapable of conversing with one another. Of course, Bloom's own assumption that Plato and Aristotle provide sufficient rationality for all occasions is also a narrow polemic; but the fear that campuses are increasingly dominated by sectarian groups that deny legitimacy to their opponents is not absurd.²⁰

Here we may detect a convergence between Bloom's views and those of the later Beard. Although Beard in 1913 ignored noneconomic sources of political conflict,

¹⁸ This paragraph develops suggestions in Daniel Walker Howe, "The Political Psychology of *The Federalist*," *William and Mary Quarterly*, 3d ser., 44 (July 1987): 485–509.

¹⁹ Wood's comment is in "Fundamentalists and the Constitution," 37. John P. Diggins's brilliant essay, "Power and Authority in American History: The Case of Charles A. Beard and His Critics," *AHR*, 86 (October 1981): 701–30, sees Beard as raising ultimate questions about the values inherent in the founding documents but then dissipating the force of his argument by seeking an "objective correlative" in property holdings. An earlier judgment, which still seems fair, was made by Richard E. Ellis in 1971: "[A]ll the serious and important works that have recently been written on . . . the adoption of the United States Constitution, even those written by Beard's severest critics, have contained points of view that are much closer to those of the Progressive historians than to those of their consensus critics." Beard was right to see that "deep divisions existed in American society, but [he] defined these divisions in a rigid and simplistic fashion"; Richard E. Ellis, *The Jeffersonian Crisis: Courts and Politics in the Young Republic* (New York, 1971), v.

²⁰ The indictment of campus factionalism is a leitmotif of *Closing*; see, for example, 313–35.

a generation later he had become uneasily aware of them, and he recoiled from their "divisiveness" in a style anticipating Bloom's. In his tract of 1934 for an isolationist-socialist America, *The Open Door at Home*, Beard devoted a chapter to the problem posed by ethnic plurality and separatist consciousness. These "factional" interests and identities might serve as a major barrier to the realization of a collectivist democracy in the United States. Indeed, by 1934, Beard envisioned the possibility of a Rousseauistic general will being created through the educational system (that eternal hope of committed teachers), now that entrenched economic interests had been undermined by the Great Depression. Both Beard in 1934 and Bloom in the 1980s argued that America needs a moral consensus, internalized into masses of citizens, that will supersede factional interests and allow the creation of a virtuous Republic.²¹ To Rousseauians, cultural pluralist arguments for ethnicity as a basic identity, inevitable and desirable, always seem deeply subversive of their intentional community of virtue. Beard and Bloom seem to differ on the place of large, propertied interests in their Rousseauian utopia, but they share a skepticism, most common outside the United States, about the destructive effect of American individualism and pluralism, of self-willedness taken as the ultimate good. Bloom is a professional student of European political theory, while Beard, though stridently and isolationistically "American" in later years, did derive much of his world view from his years in England. W. H. Auden once tried to distinguish between North American and European styles of thought. Europe, he said, believed that virtue was prior to liberty and should be enforced: "What matters most is that people should think and act rightly." The United States believed in virtue but felt it was worthless unless freely chosen, without the coercion even of a didactic education. Liberty, therefore, came before virtue. Both Beard and Bloom, however, would see liberty as valuable primarily insofar as it facilitated the achievement of a virtuous society.²²

Thus even a role model for twentieth-century antiformalists, one of the men attacked by Bloom as abandoning the necessary belief in absolute values, actually preserved his own faith in ultimate standards. As John P. Diggins has argued, Beard at heart was an idealist attempting to isolate what had gone wrong, to find the reasons why his beloved republic had not lived up to the noble ideals of the Declaration. His stress on conscious economic motives was an effort to discover an "objective" cause, which might be subject to remedial surgery, for the larger failure of vision. Whether programs of political education such as both Beard and Bloom demand are desirable or could be effective are matters of angry dispute today, reflected in controversies over English-only policies and the content of "Great Books" courses. One reason for the attention given to Bloom's book is the widespread fear that our critics are now correct—America has no common cultural ground save ambition, greed, and hostility to anything "different." The issues

²¹ Charles A. Beard, with the collaboration of G. H. E. Smith, *The Open Door at Home: A Trial Philosophy of National Interest* (New York, 1934), esp. 201–09. An invaluable account of Beard's career is Ellen Nore, *Charles A. Beard: An Intellectual Biography* (Carbondale, Ill., 1983). Nore's treatment of *Open Door at Home*, 137–53, places it in the more usual economic context that Beard imposed on his interpreters. A modern classic making this argument on the need for moral consensus is John P. Diggins, *The Lost Soul of American Politics* (New York, 1984).

²² The most succinct statement of Beard's formal philosophy of history is Charles A. Beard, "Written History as an Act of Faith," *AHR*, 39 (January, 1934): 219–27. An insightful treatment is in the essay by David Johnson, "Charles Beard and the Crisis in Thought and Economy," ms. in possession of Professor Johnson of Portland State University. W. H. Auden, in Denis Donoghue, *Reading America: Essays on American Literature* (New York, 1987), 14.

raised by America's love of change and lack of final goals are serious ones, even if the proposed remedies are Draconian or impossible.²³

While Beard's and Bloom's visions of the good society overlap, the other paradigmatic scholar singled out for attack, Carl Becker, makes a better target. Becker, the civilized skeptic who admired William and Henry James as exemplars of sophisticated understanding and whose greatest works were suffused with a sense of the transitoriness of all human goals, can be seen as offering a radical vision of mutability. Indeed, the notion of "Everyman his own historian" anticipates the views of contemporary, poststructuralist scholars. Becker was certainly the historian closest in viewpoint to one of Bloom's other anti-exemplars, the civilized skeptic, Justice Holmes. As R. R. Palmer once noted, the coherence of Becker's famous argument that the thirteenth and eighteenth-century philosophers were brothers under the skin rested on his own belief that the most basic contrast was between absolutist theories and those that recognized change and plurality. To Peter Gay, Becker's severest critic, Becker had glossed over the vital distinction between theological and naturalistic bases for belief.²⁴ And the epilogue to Becker's *Declaration of Independence* does offer a perceptive, resigned, and accepting account of the decline of belief in natural rights during the nineteenth century. Still, Bloom's indictment of Becker seems based on a special, polemical reading. When Becker asserted that it was a meaningless question whether the philosophy of the Declaration was true or false, he was not asserting the triviality of the philosophy but its unavailability to scientific test. It remained a vital "fighting faith," which Americans needed to affirm as if demonstrably true, indeed, would prove by affirming its tenets in practice. Thus there is a submerged affinity even between Becker and Bloom: both recognize the tremendous dissolving power of scientific canons of verification on traditional (or other) beliefs, but Becker responded in the tradition of William James, while Bloom insists that only pre-modern deductivism will save us. Becker's characterization of medieval rationalism catches the contrast: "[T]he function of intelligence was . . . to demonstrate the truth of revealed knowledge, to reconcile diverse and pragmatic experience with the rational pattern of the world as given by faith."²⁵

ALLAN BLOOM'S BROADEST CHARGE against "historicism" is an extension of his recoil from Becker's acceptance of change as the norm in matters of belief as in technology. Bloom insists that Becker and Beard undermined a necessary faith in national virtue by denying a fundamental axiom of the Framers themselves, that ideas were permanently true or false, based on correspondence with an unchanging Nature external to human reason. In trying to clarify the indictment, we need to separate two strands: first, the historical point, that our modern absolutists are

²³ Diggins, "Power and Authority in American History"; also see Diggins, *Lost Soul of American Politics*, which places Beard in a broader tradition of ethical concern descending from the founding.

²⁴ R. R. Palmer, "Thoughts on *The Heavenly City*," and Peter Gay, "Carl Becker's Heavenly City," in *Carl Becker's Heavenly City Revisited*, Raymond D. Rockwood, ed. (Ithaca, N.Y., 1958), 123-33, 27-51. Palmer criticized Becker for accepting such a thoroughly contextualist view of ideas as to explain them away without regard for any intrinsic or persistent value. Whether this was wholly true of Becker, it remains a strong tendency when professional historians discuss ideas, perhaps because of disciplinary differentiation as well as a Deweyite assumption that ideas reflect or react to external stimuli. A thoughtful appraisal of this "professional deformation" is in Diggins, "Oyster and the Pearl."

²⁵ Carl A. Becker, *The Declaration of Independence* (New York, 1956), 224-79; Carl A. Becker, *The Heavenly City of the Eighteenth-Century Philosophers* (New Haven, Conn., 1932), 7.

the direct heirs of the Framers, rightfully invoking Madison and Jefferson and Adams to condemn the modern academy; and second, the question of that alleged need to believe in absolute values and categories of interpretation.

The first strand is more manageable than the second, in that historians can compare Bloom's absolutist faith with that of the Framers to find similarities and differences. It resembles the second in that a solid answer would need a long book, but the two major responses can be sketched. It is true that most eighteenth-century thinkers, at least in the abstract, had an "originalist" theory of language. This theory assumed that words when coined had had a clear primary meaning, which was then covered by layers of ambiguous usage, as a result both of partisan misreading and the inevitable deviation of human understanding from the ideal. Similarly, the Framers believed in an external Nature, unchanging and knowable to us. This structure of the universe had moral value, since understanding it through research showed the direction and limits of possible change. Derived from this faith in unchanging reality and its power of moral instruction was Thomas Jefferson's belief that republican architecture should copy the examples of Greece and Rome, since Americans would aspire to virtue more easily if surrounded by a classical republican environment. Most obviously, the tradition of legal and constitutional interpretation, which quickly became the cornerstone of American political thinking, postulated the existence and authority of sacred texts, which expert practitioners applied to new situations.²⁶

It is, however, with the tradition of constitutional interpretation that the differences between the Framers' world view and that of late twentieth-century absolutists emerge. The Framers' tradition has always been broad, flexible, and responsive to context (including the demands of strict constructionists for narrowing the range of legitimate variation). Bloom blames the rise of "openness" on Justice Holmes in the 1910s, but it was John Marshall in 1806 (applying a dictum of Madison's in *Federalist* 37) who suggested that language had irreducible ambiguity and who in 1819 said, "we must always remember that it is a *constitution* we are expounding." The term "expounding" is indeed less historicist than "interpreting," but Marshall certainly did interpret, through masterful opinions that filled in the general statements of the Constitution in ways that some of the surviving Framers rejected as unreflective of their recollection of their original

²⁶ On the semantic originalism of Noah Webster, see Richard M. Rollins, *The Long Journey of Noah Webster* (Philadelphia, 1980), esp. 129–33. The outstanding re-creation of Jefferson's naturalistic absolutism is Daniel J. Boorstin, *The Lost World of Thomas Jefferson* (Chicago, 1948). A revealing, detailed study of the formation of the tradition of constitutional interpretation is William F. Swindler, *The Constitution and Chief Justice Marshall* (New York, 1978). An excellent, though somewhat uncritical, account of the "essentialist" moral system and literary criticism of its greatest eighteenth-century champion is in Walter Jackson Bate, *Samuel Johnson* (New York, 1977), esp. 296–317. Even Johnson, however, illustrates the dilemma of this essentialism based on belief in a given, unalterable human nature: it rests on a homogeneous society and system of moral beliefs, and the definitional vagueness is not seen as such since "everyone" shares similar axioms. Once change occurs, conflicting definitions of the natural order arise. T. S. Eliot believed that an essential organic stability was lost with the shift of Anglican sensibility from the balanced impersonality of Lancelot Andrewes to the personal indulgences of John Donne, while, in the twentieth century, symptoms of collapse are found in wives who wish for careers; T. S. Eliot, "Lancelot Andrewes" [1926], *Selected Prose of T. S. Eliot*, Frank Kermode, ed. (New York, 1975), 187. The question of drawing the line between "respectable" and "absurd" or "unsound" beliefs had passed to science and scholarship in the secular and pluralist society that emerged after the mid-nineteenth century. The special animus of Bloom's work can be isolated by noting that the claims of science and scholarship to establish truths beyond fashion were challenged precisely in the 1960s, as mentioned earlier. The specter of uncontrollable plurality of belief was bound to evoke demands for a return to "common sense," just as Hume's "infidelity" provoked the Scottish commonsense realism philosophy at the end of the eighteenth century.

intent. The same tradition of broad interpretation can be found among the Framers themselves. As learned men and (many of them) lawyers, they were aware that language was an irremediably “cloudy medium” and that a process of “construction” of the Constitution, in terms of a developed system of expounding legal documents, was unavoidable. The common-law tradition of constructing statutes stressed the use of preambles to ascertain a law’s intent and assumed that judges, working within a coherent legal culture that set limits, could not avoid considerable latitude in applying statutory language to particular cases.²⁷

It is true, as H. Jefferson Powell has shown in a superb essay, that there was a counter-tradition, drawn from a mix of Protestant literalism with Enlightenment belief in a universal common sense, which insisted that one plain meaning could be extracted from statutes.²⁸ But this anti-hermeneutic tradition tended to be associated with those who opposed the Constitution—sometimes on the very ground that its broad language would demand broad interpretation. Further, the very acceptance of the Constitution would suggest that a need for “construction” was also admitted. The “originalists” of the late twentieth century, whose premises Bloom shares, are arguably the descendants of a long tradition of opposition groups who insisted on “common sense” and the allegedly plain meaning of words as opposed to what they saw as the oversophisticated and self-serving interpretations offered by professional elites. Protestant versus Catholic, country versus court, Antifederalist versus Federalist, Jeffersonian versus Hamiltonian, perhaps New Freedom versus New Nationalism, now originalism versus broad constructionism in the tradition of John Marshall—these are functional oppositions imposed on American political discourse by the central power of a written constitution, whose language is characterized by an irreducible ambiguity when confronted with particulars. Both interpretive traditions involve extensive exercises in hermeneutics. This can be seen most easily in the broad tradition, but it also happens in the narrow, in the form of disputes over evidence and the nature of “intent.” Exponents of both traditions in practice, while employing historical evidence in an often-selective fashion, tend to echo the eighteenth-century scholars’ lack of interest in the subjective intentions of legislators; they prefer to confront the document with their own informed judgment of the interpretive tradition and the needs of their own time.

Turning from the constitutional tradition to more general philosophic issues, I find that the major difference between the “absolutism” of the eighteenth century and that of its twentieth-century admirers lies in epistemology, in theories of

²⁷ A searching account of the development of legal and constitutional hermeneutics is by H. Jefferson Powell, “The Original Understanding of Original Intent,” *Harvard Law Review*, 98 (March 1985): 885–948, esp. 910. For Marshall on language, in the Burr trial (1806), see Swindler, *Constitution and Chief Justice Marshall*, 177. Madison complained of the “cloudy medium” of language in *Federalist* 37; Alexander Hamilton, James Madison, and John Jay, *The Federalist or the New Constitution* (New York, 1945), 237. Marshall’s famous dictum on expounding the Constitution is from *McCulloch vs. Maryland* (1817), quoted in Swindler, 309. This long and powerful decision (reproduced in Swindler, 305–23) should be read in full to understand how sweepingly interpretivist were the decisions of one of the men now deified by originalists as axiomatic, ahistorical lawgivers. In essence, Marshall argued that any power not expressly denied in the Constitution, and remotely deducible from any clause in the document, was constructively intended by the Constitution. The clue lies in what Marshall did *not* do: he never argued for a historicist, contextualist reading of documents. And that is the essence of treason for originalists. They are prepared to allow great latitude, with considerable scope for creative interpretation, as long as the ultimate goal of one final “true” interpretation is affirmed. A superb source for the breadth and power of Marshall’s constitution-making is Swindler, *Constitution and Chief Justice Marshall*, esp. 24–85. A thoughtful account of traditions of constitutional interpretation is William M. Wiecek, *Liberty under Law* (Baltimore, Md., 1988).

²⁸ Powell, “Original Understanding,” esp. 900–13, 939–48.

knowledge. The Framers believed in a knowable Nature, the study of which would reveal the possible and its limits. But, for them, the method of investigation was empirical, an effort through observation and experiment to push back the boundaries of ignorance, which entailed a willingness to change when confronted with new data or inferences. The great modern account of this empiricist temper among the American Founders is Daniel Boorstin's *Lost World of Thomas Jefferson*, but the flavor of the original can be tasted directly in Lester Cappon's collection of *The Adams-Jefferson Letters*. Here the two ex-presidents roam excitedly over an enormous range—history, religion, political theory, personal gossip, and the latest scientific experiments. The open-mindedness of both men is striking. Adams, the humanist fascinated by semantics, celebrated the revelatory power of proto-anthropological efforts to isolate the mythic structures common to all religions. Jefferson at age eighty-two was enthusiastic about French experiments that removed the cerebellum from frogs, partly because of what they suggested about the physical causes of diverse perception and reasoning among humans. It was just this sort of intensely empiricist frame of mind that later helped to erode any larger faith in an eternal Nature. It was the belief in what Boorstin calls "givenness," combined with the empiricism, both encouraging a scorn for self-conscious theory, that left American educated thought open to change; had it been as firmly deductive as that of the Straussians, those eternal verities of Nature and human nature might have lasted intact—or been overthrown violently. Charles Darwin (the central subverter of Bloom's ideal) and then Dewey lay ahead, retrospectively, a logical development. Similarly, as Boorstin argues, the striking mutability and variety of the urban world in the years after 1870, encouraging an acceptance of change as normal, would naturally tend to erode the idea of a fixed, eternal Nature and replace it with a conception of culture as generated by the shifting needs of human beings.²⁹

THE SECOND MAJOR ISSUE raised by Bloom's indictment—the existence of a universal human need for absolute values believed absolutely—is a perennial one. Many have agreed; Hume and other gentleman-agnostics thought that organized religion was necessary to maintain fear and reverence among the uneducated; T. S. Eliot warned that emancipation from a universal faith and church would lead to people killing each other in the streets. It is Eliot, among twentieth-century conservatives, whose thought shows the most striking similarities to that of Bloom and other Straussians: the revulsion from intellectual modernism, and from corrosive social modernity (the loss of traditional hierarchy and status, the acceptance of universal mobility and equality), the need for hierarchy, for tight communities of belief and reverence led by a clerisy as the basis of civic order and personal self-development. They differ in the source of authority and reverence: for Eliot, high Anglicanism, with Christian dogma as the "rule and standard of social as well as individual life," for Bloom, the close study of classic texts to deduce authoritative understanding in a similar manner.³⁰ But not only overt conservatism shows strong parallels to

²⁹ The antiphilosophical empiricism of the Founders' philosophy and the effect of urbanization on belief are drawn directly from Boorstin, *Lost World*, esp. 111–39, 246–48; Purcell, *Crisis*, 141–57. On Adams and Jefferson, see Lester J. Cappon, ed., *The Adams-Jefferson Letters* (Chapel Hill, N.C., 1988), 605–06.

³⁰ T. S. Eliot, BBC lecture of March 1932, printed in *The Listener*, March 23, 1932, quoted in John D. Margolis, *T. S. Eliot's Intellectual Development* (Chicago, 1972), 164.

Straussianism: one finds them also on what convention calls the left of the political spectrum, among Marxian and Freudian thinkers who share the recoil from messy openness, from the lack of firm categories and identities, which they see as promoted by the pluralist liberalism popular among intellectuals. The issue is epistemological and psychological, and it divides traditional allies on the political spectrum. The most searching psychological critique is Quentin Anderson's book *The Imperial Self* (1971); the most lively polemicist is Christopher Lasch, who offers a critique of feminism and environmentalism as embracing an ideal of the "minimal self," androgynous and over-responsive, instead of the firmly differentiated identities postulated by Freud's theory of personal development. The similarity to Bloom's historical analysis is also strong: Lasch contrasts "the Aristotelian tradition of political theory," in which *phronesis* (practical reason) "describes the development of character, the moral perfection of life, and the virtues specific to various forms of practical activity," with the narrower modern view of politics, emerging in the sixteenth century, which defines "material survival, the physical maintenance of life, as the chief business of the state. From that position it was a short step to the modern conception of politics as political economy, which assumes . . . that 'individuals are exclusively motivated to maximize their private wants, desires, and interests.'" ³¹

Here we see the convergence of left and right on a view of modern liberalism and "relativism" as degraded offspring of capitalism or the loss of classical reason. The psychological convergence is also notable: Lasch's affirmation of the crucial role of guilt in the development of an autonomous self is a secular echo of Eliot's lament, "With the disappearance of the idea of Original Sin, with the disappearance of the idea of intense moral struggle, the human beings presented to us both in poetry and in prose . . . tend to become less and less real." Other analogues could be found, on both ends of the political spectrum, united by their fear of the Heraclitean flux of modernism; but the issue of the need for absolutes predates the American founding, is indeed, as William James said of the ontological argument for God, "infinitely old." ³²

The most direct anticipation of Bloom's critique came a half-century ago at the University of Chicago, when Robert Hutchins and Mortimer Adler led a neo-Thomist attack on pragmatism as a silent enemy of American democracy. Its most extreme statement was Adler's incendiary speech of 1940, "God and the Professors," which described American academics, with their "positivist" belief in value-

³¹ Christopher Lasch, *The Minimal Self: Psychic Survival in Troubled Times* (New York, 1984), 253–54.

³² T. S. Eliot, *After Strange Gods* (1934), in Margolis, T. S. *Eliot's Intellectual Development*, 173. Margolis gives a thoughtful presentation of Eliot's vision of a re-Christianized culture. For Eliot's formulation, see *The Idea of a Christian Society* (New York, 1940). All of Lasch's *Minimal Self*, but especially 197–259, develops the argument briefly adumbrated here.

There are obvious parallels between Bloom's attack on modernity and historicism and those of many other conservative critics including Irving Babbitt, *Democracy and Leadership* (Boston, 1924). To a striking degree, Straussianism can be seen as a revival of Goethe's dictum that classicism is health; romanticism is disease. Babbitt, however, classified Rousseau as an agent of infection, *Rousseau and Romanticism* (Boston, 1919). Kenneth Minogue has noted important similarities between Bloom's indictment and the Marxian critique; indeed, the Straussian analysis of the emergence of "modernity" in the seventeenth century converges with that of C. B. Macpherson, *The Political Theory of Possessive Individualism: Hobbes to Locke* (New York, 1959), as well as that of Lasch, as noted in the text. Similarly, a comparison of Bloom's vision of the curse of modernism along side that of Roman Catholic philosophy is revealing. See Kenneth Minogue, "The Graves of Academe," *Times Literary Supplement*, 4399 (July 24, 1987): 786; Gerald J. Galgan, *The Logic of Modernity* (New York, 1982); and Northrop Frye, "Antique Drum," in Harold Bloom, ed., *Modern Critical Views: T. S. Eliot* (New York, 1985), 19–30. For James's remark, see Fred Matthews, *Quest for an American Sociology: Robert Park and the Chicago School* (Montreal, 1977), 33.

neutral research, as a greater danger to democracy than Adolf Hitler, and asked whether "the tyrants of today are not like the Babylonian and Assyrian kings—instruments of Divine justice, chastening a people who had departed from the way of truth." The return to truth entailed rigorous logical reasoning from correct premises, the subordination of science to philosophy, and the quest for a new summa similar to that of Thomas Aquinas, which would incorporate the results of empirical inquiry into a total organization of human knowledge and wisdom. Adler supplied a long list of axioms, or necessary presuppositions, assent to which was required of academics who wished to participate in his model of relevant discourse. His prolific works seem to have had little explicit influence inside the universities, but they marked one extreme of a broader attack on the then-dominant paradigm of American philosophy and social science—the antiformalist frame of reference, which stressed contextual influences on thought, and the multiplicity of viewpoints from which experience could be interpreted. Peter Novick has given us a thorough account of the rise and fall of Beard's and Becker's perspectivist philosophy of history, stressing the way a need for absolute values contributed to its loss of influence after 1940. The same shift can be found in sociology, with the turn from the multi-perspectival, process-oriented theories of the Chicago school to the structural-functionalism of Talcott Parsons, which attempted to view societies from outside, through universal categories of analysis.³³

This broad shift of paradigms did not bring Mortimer Adler into the mainstream of academic discourse, partly because his demand for explicit philosophical commitment offended the self-image of scholars as Baconian empiricists. The example of Adler does, however, help us place the work of Allan Bloom, rooted in the Chicago neo-Thomism, on the philosophic spectrum. It is a location more common in Europe than in twentieth-century American academic life, although it has links with much of our popular thought, with Catholic conservatism and Protestant fundamentalism, as well as with the kind of "naive Realism" that simply assumes that the world is as it appears to us, that our own moral judgments are "natural" and written into the structure of the universe. In general terms, Bloom's position rests on explicit premises drawn from ancient Greek and medieval doctrines—from Plato, Aristotle, and Thomas Aquinas. Straussianism has a theological twist, in that its core of universal, timeless truth resides in Plato, Aristotle, and Aquinas, and for Americans, at a lower level, in the Constitution. This level is lower because the American "founding" rests on the "modern" philosophy of Hobbes and Locke, with its concern for origins rather than ultimate goals, for survival and material prosperity rather than disciplined self-development toward a final realization of the fully human being, the man of wisdom and justice. Still, as contemporary Straussians believe, enough pre-modern republican theory remained to ennoble the American founding and give it richness.³⁴

³³ Mortimer J. Adler, "God and the Professors," *Vital Speeches of the Day*, 7 (December 1, 1940): 98–103; Novick, *That Noble Dream*, 281–360; Fred Matthews, "Social Scientists and the Culture Concept, 1930–1950: The Conflict between Processual and Structural Approaches," *Sociological Theory*, 7 (Spring 1989): 106–20.

³⁴ Bloom, *Closing*, 157–79, 246–93. A searching critique of Bloom's use of classical philosophy to support his belief in "givens" is by Martha Nussbaum, "Undemocratic Virtues," *New York Review of Books*, 34 (November 5, 1987): 20–26. Bloom, *Closing*, esp. 82–137. Boorstin, *Genius of American Politics*, esp. 4–11. The treatment of the American founding as a blending of the strengths of classical and modern political theory is the work of Strauss's students; an extended analysis in this mode is Thomas L. Pangle, *The Spirit of Modern Republicanism: The Moral Vision of the American Founders and the Philosophy of Locke* (Chicago, 1988). Strauss himself barely mentioned the subject; his one brief comment (*Natural Right and History*, 245) suggests that he might have agreed more with Beard's economic reading of *The Federalist*

As their affinities with Adler's neo-Thomism suggest, the Straussians accept basic philosophic doctrines far removed from those of most American academics—that ultimate reality lies in essences, abstract categories, of which the specific examples we encounter in daily life are only imperfect copies. Our daily experiences are to be interpreted and judged in terms of criteria set by the abstract but “real” universals. Thus much of Bloom's indictment of the decadent, liberal, late twentieth century rests on our loss of virtue in failing to live up to these universals—we no longer have fully developed careers because of too many crosscurrents, our families are damaged by the refusal of women to conform to their natural role and the consequent failure of men to be dynamic, masterful, competent breadwinners and disciplinarians. In politics, we have lost interest in the ultimate goal of a just society and in the educative function of political participation that the Framers thought vital; instead, we have substituted an amoral fascination with the process itself, the competing interests and their tactics. Bloom's jeremiad sees the declension from classic virtue as the result of the infection of innocent, pre-theoretical America, with its “givenness” of belief, by alien doctrines spread by “intellectuals” from Dewey and Holmes to the disciples of Jacques Derrida and other poststructuralists.³⁵

Some historians will find fault with Bloom's exact diagnosis of the disease; as an expert on European philosophy, he singles out Nietzsche and later German philosophers as the fatal influences. This analysis is plausible in the “mountain-goat” theory of intellectual history in which Nietzsche represents the highest development of a paradigm, whose tendencies can also be found in other, apparently distinct, traditions. As Mortimer Adler reminds us, however, the skepticism and relativism that terrify Bloom were common among college students by 1940, before the postwar popularity of such thinkers. The longer-term influence is the tradition of Anglo-American philosophy, the nominalism of John Stuart Mill and the pragmatism of James and Dewey, which set the framework not only for the social sciences but also for the public school curriculum from the 1910s onward. This tradition assumes that ultimate reality lies in historical particulars: individual human beings and groups in their uniqueness rather than as imperfect copies of universal or “natural” necessities. Joined to this nominalist viewpoint is the cultural pluralism associated with the Columbia anthropology of the 1920s and 1930s—Ruth Benedict, Margaret Mead: each distinct group has its own culture, its envelope of meaning and practices, its unique ways of adapting to the challenge of survival and self-development. From this philosophical position, it is pointless to talk of our own times as “decadent,” since we as unique individuals have a greater variety of choice, of directions in which to try to construct satisfying lives out of the materials offered by a tremendously rich and complex environment.³⁶

than with that offered here or those of his own disciples. A pragmatist's analysis of the Straussian philosophy is offered by Richard Rorty, “That Old-Time Philosophy,” *New Republic*, 198 (April 4, 1988): 28–33; see also the vigorous rejoinder of Harvey C. Mansfield, Jr., “Democracy and the Great Books,” *ibid.*, 33–37.

³⁵ Bloom, *Closing*, 126–27.

³⁶ Mortimer J. Adler, “This Pre-War Generation,” *Harpers*, 181 (October 1940): 524–34. An attempt to place Columbia anthropology in its ideological (as against its academic) context is Fred Matthews, “The Revolt against Americanism: Cultural Pluralism and Cultural Relativism as an Ideology of Liberation,” *Canadian Review of American Studies*, 1 (Spring 1970): 4–31. There are serious issues here, since later anthropologists have attacked Columbia anthropology for seeing, or overstating, what it was eager to find. Derek Freeman, *Margaret Mead and Samoa: The Making and Unmaking of an Anthropologist*

Another line of objection to Bloom's historical account of the triumph of "historicism" could be argued at length. He argues that the antiformalists, Dewey and Holmes, Beard and Becker, helped to create a receptivity to relativism, even if they did not consciously desire to destroy all standards of judgment. To assert this, however, is to read Dewey and Beard as though they were identical in influence to such apostles of mystical individualism, of the repeal of all "givens," as Henry James, Sr., Walt Whitman, and Allen Ginsberg. Indeed, some of Bloom's anger seems to be directed at a campus and celebrity culture that could accept a Whitman or a Ginsberg as a possible model. While it is possible to envision a serious conversation between Ginsberg and John Dewey, the difference of viewpoints is profound. Although strongly opposed to absolute ahistorical standards, Dewey believed that a cooperative application of scientific method through public discussion would generate contextually "objective" knowledge of the best feasible solution to contemporary problems for a particular community with particular values and goals. Sidney Hook dubbed this position "objective relativism," although "objective" may have misleading connotations, since its achievement would have involved compromise and tolerance as part of a common quest to define inclusive values as well as viable techniques. Although he imputes a never-ending multiplicity of standards that Dewey would have regarded as a caricature of his thought, Bloom was not wholly wrong to sense a tendency toward relativism in the thought of pragmatists. Dewey did crusade against inherited arbitrary standards, and observers have felt an elective affinity between the apostles of boundlessness and self-creation and the process model in pragmatic philosophy and social science. And it is true that the radical empiricism of William James—with its belief that general categories are useful tools for organizing and acting on experience but are themselves part of experience and cannot by themselves limit or proscribe it—has important affinities with such recent concepts as labeling theory in sociology, which emerged from the process model after it was detached from Dewey's faith in the one-best "adjustment" and in a single, coherent American society as the unit to whose health and development thought was dedicated. Bloom's account, in other words, is perceptive though truncated: there was a slippery-slope effect, by which pragmatism led to "relativism," or at least to a more inclusive pluralism, as an alienated romantic temper, intensely aware of power in human relations and reading "reason" as "being reasonable," rejected the element of compromise inherent in Dewey's "method of intelligence."³⁷

(Cambridge, Mass., 1983). For an overview of this controversy, see Jane Howard, *Margaret Mead: A Life* (New York, 1984).

³⁷ On Dewey's ethics, the most convenient source is Sidney Ratner's collation of selections into a treatise: *Intelligence in the Modern World: John Dewey's Philosophy* (New York, 1939), esp. 751–93. Hook develops his concept of Dewey's position as one of "objective relativism" in "Closing of the American Mind," 127–30. The term "objective" may, however, suggest a harder, more rigid notion of one "correct" solution to every practical problem than Dewey's own work would suggest. See also Sidney Hook, "The Desirable and Emotive in Dewey's Ethics," in *John Dewey: Philosopher of Science and Freedom*, Sidney Hook, ed. (New York, 1950), 194–216; and Sidney Hook, "Pragmatism and the Tragic Sense of Life," in *Contemporary American Philosophy*, 2d ser., John E. Smith, ed. (London, 1970), 170–93, esp. 171, 182, 190. The magisterial work of James T. Kloppenberg, *Uncertain Victory: Social Democracy and Progressivism in European and American Thought, 1870–1920* (New York, 1986), now presents Dewey's thought in broad philosophical and political contexts, helping to clarify the distaste felt for it on both right and left. On the movement from process sociology to labeling theory, see Fred Matthews, "Ontology and Chicago Sociology," *Philosophy of the Social Sciences*, 15 (June 1985): 197–203. The classic attack on American romanticism's rejection of limits on the possible is Quentin Anderson, *The Imperial Self: An Essay in American Literature and Cultural History* (New York, 1971). Better terms for the philosophical position of the antiformalists might be "perspectivist" or "contextualist"; as Peter Novick

This empiricist and nominalist temper is completely distinct from that of Allan Bloom. For Bloom, science, research, and actual lived experience are trivial, because truth comes deductively from the sacred texts of ancient thinkers. From this perspective, the starting point, the initial axiom, is our absolute need to believe in absolutes; anything threatening that necessary belief is false because it threatens evil consequences. We can now appreciate one of the ironies of Bloom's argument—despite his disdain for pragmatism, his argument is a pragmatic one, resting on the alleged human need to believe in absolutes. A large question remains: whether research scholarship, if it tried, could through empirical methods establish or disprove the kind of “natural” or structural limits on human variability that Bloom believes essential. Sociobiology and comparative anthropology come to mind as possible methods, yet neither figures largely in contemporary discussion or in Bloom's book. Given the strength of his alarm, it is clear why Bloom would dismiss obvious enemies like cultural anthropologists on personal grounds. But his enemies really are legion, since his book implies a need to subordinate all of modern art and scholarship to the authority of *a priori* absolutes from which they escaped over the last two hundred years. On the explicit level, the notion of “openness,” encouraged by modern “historicism,” is the evil explosive force; implicitly, however, the indictment of the modern university centers on the ideal of empirical research, freed from subordination to absolute values. Bloom is in the tradition of his master, Plato, who condemned poetry for inflaming the imagination and subverting necessary mental discipline; but the interdict would now apply to empirical scholarship as well, unless it agreed to operate within given categories derived from classical philosophy. And it is likely that scholarship, because of its increasing doubts about reaching that one certifiable “reality,” has weakened its ability to resist Bloom's pragmatic indictment of pragmatism and historical indictment of historicism.³⁸

WHAT ANSWER MIGHT BELIEVERS in “historicism” return to the charge that they have helped to close some minds in America? Is there, first, any relationship between a specific academic doctrine and popular culture? Even granting the premise that academic modernism matters, Carl Becker might have replied to Bloom that we are locked into a spiral of intellectual history, in which philosophical opposites alternate as the inadequacies of each become apparent. Empiricism, with its championing of the singular and the various, undermines belief in categorical truths, which large numbers of people feel are vital to social order and therefore in need of reassertion. As Dewey said, the real enemy of absolutism is the process of thinking itself, which tends to subvert conventional wisdom: “Let us admit the case of the conservative: if we once start thinking no one can guarantee where we shall come out, except that many objects, ends and institutions are doomed. Every thinker puts

points out, “relativism” is a polemical term used by absolutists to stigmatize anyone who doubts the possibility of a single timeless truth; Novick, *That Noble Dream*, 3, 3 n.

³⁸ This sketch cannot deal adequately with the major issues raised by the contrast between “empiricist” and “deductivist” theories. Much of modern social science, especially that concerned with guidance for roles in complex societies, has been “deductivist” and “definitional,” at least in its presentation to the public of guides to proper conduct; but it would claim to be based on an empirical study of human nature and society of the type praised by Jefferson. For an effort to deal with the dilemmas of using social science as a source of prescriptive norms, see Fred Matthews, “In Defense of Common Sense,” in Jack Salzman, ed., *Prospects Four: An Annual of American Studies* (New York, 1973), 459–516.

some portion of an apparently stable world in peril and no one can wholly predict what will emerge in its place."³⁹

As noted, the question of whether or not thorough research could help to reestablish the absolute limits on human variability for which Bloom yearns remains unanswered. Without that breakthrough to timeless knowledge, Richard Rorty argues, "the general adoption of antimetaphysical, antiessentialist views about the nature of morality and rationality and human beings" would not "weaken and dissolve liberal societies," since the decline of religious faith among the educated in the nineteenth and early twentieth centuries did not lead to the loss of self-control and self-sacrifice so feared by conservatives. Conservatives would disagree, pointing to the rise of violence and "permissiveness" as consequences of a culture that loosened necessary norms. The debate is as old as it is intense. There is in *Closing's* argument a disturbing tone reminiscent of Dostoyevsky's Grand Inquisitor, who demanded that the masses be forced to abandon any quest for transcendence of the limits set by authority, because conformity protected the soul against doubt and supported social stability. It is an index of the strength of Americans' recoil from the disturbing effects of the contemporary academy that such a program would become a best seller.⁴⁰

³⁹ Ratner, *Intelligence in the Modern World*, v.

⁴⁰ Richard Rorty, *Contingency, Irony, and Solidarity* (Cambridge, 1989), 85; Fyodor Dostoyevsky, *The Brothers Karamazov*, trans. Constance Garrett (New York, n.d.), Book 5, chap. 5.

Reviews of Books

GENERAL

PETER GAY. *Freud: A Life for Our Time*. New York: W. W. Norton. 1988. Pp. xx, 810. \$25.00.

Peter Gay's biography of Sigmund Freud is a tribute from one representative of the spirit of Enlightenment to another. Author and subject share an identification with the liberal rationalism of European bourgeois culture, whose inherited limitations and restrictions Freud sought to combat with the weapons provided by its self-critical commitment to truth. Gay's Freud is (as Freud said of himself) a scientific "conquistador," his life the story of his great discoveries but also of his never completed "struggle for self-discipline, for control over his speculative impulses and his rage" (p. 316).

There is much to be grateful for in this book. It will surely be—as the subtitle proclaims—our time's life of Freud. The scholarship is prodigious, both in published and manuscript sources, the writing elegant and precise, the sweep grand. Gay corrects many errors and provides new—often-illuminating and arresting—details. The Freud who emerges is a real flesh-and-blood figure, beset by conflict, sometimes unable to meet his own standards of analytic distance (as in his analysis of his daughter Anna), jealous and generous, narrow-minded and visionary by turns. The author's commitment to psychoanalysis does not blind him to Freud's limitations and excesses: his narrow appreciation of artistic imagination, the speculative overreaching that posited the death instinct, the limited and self-serving understanding of femininity, the aggression (against each other as well as their enemies) that too often inspired the analyses undertaken by Freud and his first followers. The text is not encumbered with academic controversy, but the extensive bibliographical essay provides both a rich survey of the relevant literature and a clear statement of where Gay stands in regard to it. Scholarly readers may sometimes find it even more absorbing than the text itself.

This is a Freudian biography of Freud in two senses. Gay never hesitates to attribute thinking or behavior he judges insufficiently rational to Freud's own (or Josef Breuer's or Carl Jung's) unresolved psychic (most often Oedipal) conflicts, finding even the anthropological fantasy of *Totem and Taboo* rooted in its author's pow-

erful struggles against his father. Used in this way, psychoanalysis continues to serve—even against itself—the critical function that makes it a descendant of Enlightenment rationalism. But the ideas and practices Gay puts into question are never ones essential to psychoanalysis as it applies to individuals: most are rejected by present-day Freudians and were even questioned by some of the orthodox from the start. Sometimes the critical attitude Gay takes up in regard to Freudian excesses even serves to defend Freud; thus, the circumstance that all of the early Freudians were given to quick, sometimes aggressive analyses of one another's slips and conflicts means that "Freud was right to think that Jung's response to his interpretation of [such a] slip was out of all proportion and thus highly symptomatic" (p. 235).

Perhaps surprisingly, applying a Freudian method to Freud's life means there is less mystery for the biographer—and the reader—to unravel than one expects from psychological biography. There is no "figure in the carpet" to puzzle out, because the shape of Freud's psychic development has to follow the general lines he worked out himself (even if he was never able fully to resolve the conflicts he recognized). "Psychoanalytic interpretation is a subversive reading," Gay tells us (p. 238), but the subversion encounters its limits when applied to the master's own life. One finds more of the drama that comes from the teasing out of a hitherto invisible pattern of significance—the drama so powerfully present in Freud's own case histories—in other, less orthodox accounts, such as those by William McGrath, Paul Ricoeur, Marthe Robert, or Frank Sulloway. This is not to say that Gay is unaware of what one might call non-Freudian conflicts in the master's life; he specifically draws attention to those between bourgeois respectability and scientific radicalism, between speculative ambition and empirical responsibility, between therapeutic concern and the Faustian aspiration to knowledge, between the uniqueness of individual experience and the claim of universal categories to encompass it. But, unless I misread him, Gay regards these conflicts only as the framework within which the discovery of psychoanalysis emerged; they provide no independent clues to the historical meaning and impact of Freud's work.

Gay's book contains much of the history of Freud's

time, but his Freud is a figure little illumined by comparison with the surrounding life of culture and politics, one who pursued his research and conjectures "in the austere isolation of his consulting room" (p. 130). Yet Gay largely accepts Stefan Zweig's judgment that Freud's importance lies more in the philosophical and moral impact of his views outside the circle of his followers than in the therapy he evolved (p. 457). Given this agreement, the kinds of attempts to elicit the cultural meaning of Freud's work out of conflicts between scientific and humanistic images of mental activity, which one finds, for instance, in Ricoeur's *Freud and Philosophy*, or out of the relations between psychoanalysis and the vicissitudes of liberal culture, as attempted by Carl Schorske, seem more relevant than Gay allows, even if one rejects specific features of such interpretations. One might also have wished for more attention to the wider cultural or philosophical implications of important turnings in Freud's thinking, for instance, the altered image of individual activity and passivity created by the abandonment of the seduction hypothesis in the late 1890s or the changed relationship between erotic energy and social life implied by Freud's positing an independent instinct of aggression, and a wider conception of eros as a counter to it, in the 1920s. But, in the face of all that Gay has achieved in this book, perhaps we should simply be grateful to him for leaving the rest of us things to think about.

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PETER DEAR. *Mersenne and the Learning of the Schools*. (Cornell History of Science Series.) Ithaca: Cornell University Press. 1988. Pp. xiii, 264. \$28.50.

Marin Mersenne, 1588–1648, was a fascinating figure who played a vital role in the development of modern science and philosophy. His correspondence, publications, and involvement in the research of people such as Descartes, Hobbes, Herbert of Cherbury, La Peyrere, Galileo, and Pascal make him an intriguing figure to study. The first modern account of him, the abbé Robert Lenoble's *Mersenne ou la naissance du mecanisme* (1943), portrayed him as setting forth a model of modern science, a mathematically described mechanical world, a hypothetical science without any metaphysical foundation. I followed with my *History of Scepticism* (1960), showing Mersenne as a "mitigated skeptic," who developed a way of admitting the force of the revived Pyrrhonian skeptical arguments and of defusing and evading their force by pointing to the vast amount of scientific and mathematical information that was in fact known.

Peter Dear, in this study, seeks to show that Lenoble and I presented too limited a picture of Mersenne's achievement because we did not show "the extent to which Mersenne's thought followed patterns found in the school doctrines he had received from the Jesuits"

(p. 27) and from late Renaissance humanists. Dear points out that Mersenne took over the moderate skeptical stance of humanist dialecticians and some Jesuits, their Ciceronian probabilism, and their restriction of certain knowledge to only a few areas. Mersenne used this and a rejection of essentialism, the view that we can know the essential natures of things, to formulate his positive program of natural philosophy. Dear carefully shows how the various parts of Mersenne's view of mathematics and science appear in scholastic writings of the period. Mersenne's conception grew out of discussions of the time. He developed a metaphysical justification, in scholastic terms and doctrines, for the use of mathematics in science, although our "ignorance of essences still stood in the way of a thoroughly satisfactory knowledge of the physical world" (p. 78). There could be true science, mathematically stated, but one could not know whether or not it related to exterior things.

The "mitigated skeptical" account of science is then traced through Mersenne's concerns regarding universal harmony, music, and mechanics. The relation of his views to Augustinian Platonic as well as Aristotelian sources is treated. Dear then compares Mersenne's views with some of his non-Aristotelian contemporaries, from philologists, such as Joseph Scaliger, to Neoplatonic, Kabbalistic thinkers, to strange thinkers such as Comenius, and "new" thinkers such as Francis Bacon. Mersenne's basic view that one could not know the essential natures of things kept him from embracing any philosophies as more than working hypotheses at best.

His mitigated skepticism also led him to a broad tolerant outlook that could encourage a wide range of philosophical, scientific, and theological thinkers of varying persuasions and religious affiliations. He could aid and abet Descartes, Hobbes, Pascal, La Peyrere, Comenius, and Galileo, while attacking and destroying the credibility of what he regarded as tools of heresy, naturalism, magic, and skepticism. (But, for all of Dear's good will in going over this, he really does not account for how Mersenne missed the irreligious potential of Hobbes's views or the super-heretical points in La Peyrere's ideas.)

Dear ends saying that Lenoble and I identified Mersenne's goals and the way he tried to achieve them. What we did not do, and what Dear does very well, is show how Mersenne's ideas, his agenda, related to his cultural environment. He was part of the continuing Catholic intellectual world, used many of its concepts, and applied them to the changing role of knowledge of the natural world of the seventeenth century. Dear points out that Mersenne never cast himself as an innovator, yet his views and his intellectual friendships were of great importance in the developments that took place.

Dear's book greatly enriches our knowledge and understanding of Mersenne and of the intellectual issues of his time. This work along with other new studies of Gassendi may make clearer the character of

the great intellectual changes that took place in early modern intellectual history. And it deepens our appreciation of the many kinds of skepticism that influenced those changes.

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ANNE HARRINGTON. *Medicine, Mind, and the Double Brain: A Study in Nineteenth-Century Thought*. Princeton: Princeton University Press. 1987. Pp. xiii, 336. \$39.50.

Anne Harrington has written a spectacular book about the attempts of nineteenth-century researchers to understand the puzzling observation that *Homo sapiens* possesses two cerebral hemispheres. At first glance they appeared identical. Were they comparable to our two kidneys, two similar organs that labor together but essentially serve to provide us with spare functioning capacity?

Harrington has produced a carefully researched and clearly presented account of the attempts to investigate the fundamental meanings of this duality. Her book starts with a chapter surveying the subject before 1860, while most of the remainder is devoted to the next forty-five years. Who are the figures most important to her work? An index name count reveals the leader to be John Hughlings Jackson, who received twice as many citations as his nearest competitor, Paul Broca. Broca is closely followed by Jean-Martin Charcot, while slightly further behind are Sigmund Freud, Charles-Edouard Brown-Séquard, and Carl Wernicke.

This book is an informed study of neuroscience, but, at the same time, the author brings in a wealth of related topics. Her focus is on "conceptual history" and incorporates perspectives from French scholarship, but she also introduces a wide variety of information and knowledge about a topic that has received little attention from historians. The first chapter reviews the early writings of a number of phrenologists and neurophysiologists. These include the work of Franz Joseph Gall, who concluded that the double brain provided the safety of reserve tissue. This view also allowed Gall to preserve the idea of a unitary soul, because he did not wish to challenge traditional religious beliefs. The Catholic church rewarded him by placing his writings on the forbidden list and denying him a Christian burial. Harrington then reviews the studies of François-Xavier Bichat and Jean-Pierre-Marie Flourens before turning to the publications of Henry Holland and Arthur Ladbroke Wigan in the early 1840s. A hiatus of research and writing followed, which lasted until 1861 when Broca discovered a speech center in the left frontal lobe of the cerebrum and proved the existence of brain lateralization.

Broca's efforts provoked considerable scientific excitement, which led to further speculation and re-

search. The two brains were obviously not identical. Attempts to clarify the issue ranged from microscopic exploration of cells, their clustering into centers and the intercommunicating fibrous tracts, to examination of the broader characteristics of human behavior in different races, sexes, nationalities, and styles of behavior. These inquiries led to the conclusion that the left brain was superior. The inferior right brain was animal-like, operated on passions and instincts, was less conscious, more feminine, and non-white, and was associated with criminal behavior and madness. Corrections to this view gradually took place toward the end of the century. Pierre Janet reminded his readers that every human had two cerebrums but that did not mean all were somnambulists, mediums, or mad.

There is much information in this book. Hysteria, for example, played an important role in these studies. Clinical inquiry revealed that hysterics had significantly more signs and symptoms on the left side of their bodies, thereby indicating problems in the right hemisphere. Charcot had many female hysterics who responded to hypnotic suggestions directed to just one hemisphere. At the other end of the spectrum, physicians searched for patients who had physical diseases of the corpus callosum, the main interconnecting fibrous bundle between the two hemispheres.

Jackson brought his own skepticism to the questions and built a neurological edifice on the proposal of a comprehensive sensory-motor reflex response in the central nervous system with both evolution and dissolution represented. Harrington concludes her main text by considering how this line of inquiry influenced Freud. Her last chapter looks to the future by presenting a brief survey of the twentieth century with its hiatus of thirty years on the topic of the double brain. In the early 1960s a resurgence of research occurred through the work of Joseph Bogen, Roger Sperry, Norman Geschwind, and Michael Gazzaniga, among many others. Harrington has given us an excellent conceptual history set in a context of social developments and including many broad topics, such as the soul, education, language and thinking, the occult and philosophy.

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JACK RALPH KLOPPENBURG, JR. *First the Seed: The Political Economy of Plant Biotechnology, 1492-2000*. New York: Cambridge University Press. 1988. Pp. xviii, 349. \$37.50.

This is an important book that needs to be read by a wider spectrum of readers than will probably notice it. The title masks significant work analyzing the implications of several recent scientific developments. The book is also freighted with a political message, which receives more explication than is necessary. Persons interested in problems that may be posed to the world food supply as the population curve nears the produc-

tion curve over the next quarter century and individuals concerned about Third World relations are among those who should read this book. Others include agricultural historians, geneticists, policy makers, and concerned citizens.

Jack Ralph Kloppenburg, Jr., provides a number of very useful judgments about the origins of plant germplasm and about modifications now being applied to plants through biotechnology. His point that much of the world supply of germplasm came originally from the Third World is interesting historically, but the events he describes cannot be changed or reversed. More important are his sections dealing with modifications to the supply of germplasm, especially since the knowledge explosion created by the discovery of DNA and recombinant DNA. That knowledge has transferred ownership of germplasm increasingly into the hands of large plant breeders. Growers now have much less control of production, as plants are remodeled to meet real or fancied demands of the market. The role of governments in plant breeding has diminished significantly.

In fact, seeds can now be produced that will give only sterile seeds so that only one crop can be produced. The farmer or grower who wishes to put aside seed for the next growing year can be thwarted entirely. Although Kloppenburg is dealing here almost entirely with plant germplasm, other matters such as meat and fibre production may be influenced, especially through cloning. Some recent advances, such as the use of ice nucleating bacteria and other microbial changes, may modify agriculture almost completely.

Kloppenburg deals well with those matters. Politically he tends to put matters in nationalist terms, although it seems to me that a better way to approach them is from the point of view of our species. Decisions about biotechnology are far too important to be trusted to scientists in their laboratories or even to historians at their word processors. Those are matters that the Food and Agriculture Organization or other agencies of the United Nations need to ponder. Kloppenburg does us all a great service by his work. One wishes for a bit less economic jargon and a simplified scientific approach. Chapter 8 of his book is the most important, as it raises ethical considerations that may arise from the genetic alteration already underway.

I work at a university that has a long history in genetics research. Before 1920 such individuals as Raymond Pearl and Karl Sax asked many of Kloppenburg's questions, within their knowledge bases, as they worked on oats, beans, wheat, chickens, and dairy cattle. It is too bad that Kloppenburg did not note some of their work. Having said that, however, let me reiterate that this is important work that deserves wide discussion. Forget the political message and go to the human message, which is the real meaning of this book.

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JAN SAPP. *Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics*. (Monographs on the History and Philosophy of Biology.) New York: Oxford University Press. 1987. Pp. xvi, 266. \$35.00.

During the past twenty years, historical studies of genetics have focused on the rise of the Mendelian theory of chromosomes, population genetics, the evolutionary synthesis, and the transformation of chromosome theory into the nucleic acid theory. Standard accounts of genetics assume that Mendelism was generally accepted by 1915, but many biologists continued to challenge or reject this approach to heredity. According to Jan Sapp, the major threat to Mendelian genetics and the new evolutionary synthesis came from advocates of cytoplasmic inheritance.

Certainly these disputes about the adequacy of the theory of the gene to explain variations, differences between species and higher-order taxa, and embryological development are fascinating aspects of the history of genetics. Sapp is primarily interested, however, in demonstrating that scientists are engaged in power struggles within and among scientific disciplines for the authority to define their field and control institutions. Although he never completely loses sight of the experimental basis of science, Sapp emphasizes episodes in which scientists presumably acted in relation to peers who granted recognition rather than in response to nature.

Although historians of genetics and evolution presumably neglected cytoplasmic inheritance in order to construct a tidy synthesis, "nucleocentric geneticists" were well aware of research on cytoplasmic inheritance and persistently attacked and criticized it. Sapp argues that the "nuclear monopoly" (p. xiv) pursued a deliberate strategy of gaining scientific authority by excluding competing theories and research programs. The kinds of experiments used by advocates of cytoplasmic inheritance were tedious and technically difficult; the results were often ambiguous. Unfortunately, Sapp provides no specific analyses of the data. He even refers to the use of experimentation by geneticists as a "polemical tool" used as a ploy to suppress competing theories (p. 43).

After World War I, geneticists in the United States became leaders in Mendelian studies. As T. H. Morgan attempted to extend the principles of genetics to the problems of development, he sometimes asserted that, in terms of heredity, the cytoplasm could be totally ignored. The degree of control Morgan's school exerted over genetics and the academic marketplace seems markedly exaggerated, however. In the 1940s French and American scientists returned to the search for examples of cytoplasmic inheritance. Boris Ephrussi's work on the respiratory-deficient "petite" mutations in yeast provided the first genetic evidence of mitochondrial heredity.

American protozoan geneticists, such as H. S. Jennings and T. M. Sonneborn, continued work begun by Victor Jollos but were more cautious in challenging

genetic orthodoxy. Sonneborn's studies of non-Mendelian inheritance patterns, including the infamous "killer trait," provoked intense interest and controversy. Ironically, Sonneborn was president of the Genetics Society of America at the time of the Lysenko affair. Many critics attempted to link cytoplasmic genetics with Lysenkoism, making the study of cytoplasmic inheritance not only anti-Mendelian but also pro-Soviet. Sapp's account of Sonneborn's career is a particularly valuable contribution to the history of biology.

In his conclusion Sapp asserts, "As we have seen, it has been difficult for us to decide whether scientific knowledge claims are accepted because those who uphold them have an interest in truth or whether they simply uphold the truth which suits their interest" (p. 160). Perhaps Sapp has seen that difficulty, but I have not. He also asserts that geneticists were afraid to look at the cytoplasmic idea. Perhaps they just saw more exciting problems with the nuclear approach. If Mendelians were wrong because they could not understand the complexity of the genetic apparatus, that does not mean that the cytoplasmic school was right any more than criticism of Charles Darwin's *Origin of Species* (1859) proves that "special creationism" is a valid scientific theory. In this finely researched study, Sapp has rescued many fascinating scientists and their struggles with ideas, institutions, and competitors from the oblivion into which narrowly focused histories of Mendelian genetics have cast them. This book, however, may say more about struggles for authority in the history of science than in genetics.

LOIS N. MAGNER
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ALBERTO GIANQUINTO. *Storia e Scienza*. (Collana di studi, number 3.) Milan: Marzorati. 1985. Pp. 293.

A ghost haunts the pages of this book, the ghost of La Place's demon. It is not a work for historians who regard their discipline as a way of inquiring into the puzzles and particulars of bygone ages; rather, it is a treatise on history as a "science" according to the lights of post-Marxist sociology and a very eclectic variety of the analytical philosophy of history. Alberto Gianquinto, invoking an impressive range of references from English, German, French, and American scholars (including Karl Popper, Jean Piaget, Ernest Nagel, Thomas Kuhn, Jürgen Habermas, and J. H. Steward), is concerned with questions of mechanism, biologism, evolutionism, functionalism, and materialism, causality, and putative "laws," covering, universal, and otherwise. The goal finally is an elaborate systems theory formulated in terms of a kind of scientific, sociological, or scholastic algebra ("c. i." = initial conditions, "CN" = necessary conditions, "SC" = sociocultural variables, and so forth), which reduces history to data prepared for formal theorizing.

Gianquinto hopes to move beyond Marx and Darwin—beyond the limitations of "classical historical ma-

terialism" and vulgar evolutionism (ideas of simple stages)—through a more complex, cybernetic sort of systematics that introduces concepts of culture, institutions, standards of living, information theory, and "feedback," as well as psychological stimuli, acquisition, and production. In this way he hopes to make the individual the object of explanation and avoid the threat of relativism without reducing individual liberty. The pivotal conception—central theoretical conceit—of Gianquinto's system portrays the field of historical explanation as a circular grid of overlapping subfields defined in terms of techno-economic, social, and cultural behavior. The resulting model may be visualized as a pizza with three toppings, each distributed over half the surface, with each of six wedges displaying the possible combinations, moving clockwise, of economic, economic-cultural, cultural, sociocultural, social, and socioeconomic factors.

This is an ingenious and learned book filled with manipulative applications of the ideas of biologists and mathematicians as well as social scientists, with social-scientific jargon and algebra, and with abstract, Rube Goldberg-like diagrams that incorporate a wide range of constants and variables, but for historians it may seem to close more doors than it opens. In general, "history" is emptied of content, analyzed into logical units, arranged in formal categories, reformed into explanatory models, and subjected to a naturalistic sort of closure that leaves the historian without a function, except to provide further raw information for processing and, presumably, programming—for answering predetermined questions but not for asking new ones, for manipulating "facts" but not for examining evidence. In terms of the analytical philosophy of history, the work raises the stakes and the level of discussion; for the study of history, however, it is still the sort of book that gives theory a bad name. For historians, serendipity still lives.

DONALD R. KELLEY
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MICHEL DE CERTEAU. *The Writing of History*. Translated by TOM CONLEY. (European Perspectives.) New York: Columbia University Press. 1988. Pp. xxviii, 368. \$40.00.

The crowning work of the late Michel de Certeau is this volume of essays on historiography (an oxymoron in de Certeau's canny vocabulary of paradox), first published in French in 1975. A second, revised edition appeared in 1980. Tom Conley has now translated the text into English, with loving fidelity to de Certeau's mellifluous Gallic idiom. This book is a brilliant, disjointed, baffling work, brimming with complex metaphors, Franco-German metaphysics, and a postmodern sensibility that blends Sigmund Freud and Jacques Lacan with the slashing critical intelligence of Michel Foucault, Roland Barthes, Jean Baudrillard, and Emile Benveniste.

Readers ill at ease in such luminous company may not understand a word of it.

One key to the book is the frontispiece, an allegorical etching from the early seventeenth century, which represents rational European man confronting a naked female American savage, who rises from her hammock to greet her conqueror. The Old World meets the New, but the etching also reproduces the experience of all Western historians since Niccolò Machiavelli. In de Certeau's thought, history, once the organic memory of a people, is now a series of encounters with otherness, with death and the dead, in which the scholar-prince creates new mythologies that proclaim irrevocable ruptures in time but also seek to heal them with "scientific" recapitulations of the past. Every work of history is an attempted conquest of the *réel*, a Lacanian term used by de Certeau to signify the sensed reality that cannot be rendered in words and is always inescapably perceived through the prisms of cultural codes. Such conquests are not literally possible—hence, the oxymoron of "historiography," which is to say, writing the unwritable—but modern Western scholars persist in trying.

Another key to de Certeau, noted by Conley, is the untranslatable title of his book. *The Writing of History* does scant justice to *L'Écriture de l'histoire*. In French, *l'écriture* means not only "writing" but also "Scripture" in the biblical sense and "literature, 'writing,' in an imperiously intransitive form" (p. xx). De Certeau, well known for his studies of religious history and secularization, views secular historiography in the light of millennia of Jewish and Christian scripturalism and its modern counterpart, the word-worshipping literary culture of Western Europe.

But this volume is also very much a postmodern book. The productions of historians, de Certeau tells us, are not to be taken at face value. They are texts, myths, fictions, describing "events" conjured out of codes. So much for the old self-styled scientific history, whose program survives today, de Certeau observes, only in the writing of popular history.

The author pursues his themes through four groups of essays, comprising, in his own words, "a fragmented discourse fashioned from tactical investigations each obeying specific laws: a socioepistemological approach (in part I), a historical approach (in part II), a semiotic approach (in part III), and a psychoanalytical, Freudian approach (in part IV)" (pp. xxvi-xxvii). The most substantial essays are perhaps the first two, on the historiographical enterprise, together with the fourth (on the transition to the Enlightenment), the fifth (on a sixteenth-century explorer's memoirs of Brazil), and the ninth (on Freud's *Moses and Monotheism*).

But back to the frontispiece, with its vision of nude corporeal America hailing clothed intellectual Europe. Faced with yet one more coruscating volume of French critical theory, the American scholar (another oxymoron?) may be tempted to draw a second significance from the etching. De Certeau's *L'Écriture de l'histoire*, taken in conjunction with all of the other invading European texts in our post-1945 academic life, from

those of Fernand Braudel to those of Jacques Derrida, is emblematic of a new European conquest of America, a conquest now of mind and consciousness. There are American theorists of historiography, for example, Hayden White, who can bear comparison with de Certeau, but not many. And, as de Certeau's abundant footnotes make all too clear, they wield little or no influence on their European conquerors.

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S. H. RIGBY. *Marxism and History: A Critical Introduction*. New York: St. Martin's. 1987. Pp. 314. \$37.50.

This book examines the nature and utility of the theory of history developed by Friedrich Engels and Karl Marx and is thus part of a continuing discussion among interpreters of Marxist historiography. It is an erudite and important contribution to that discussion.

S. H. Rigby argues that the Marxian legacy of ideas is ambiguous in that Marx very clearly advanced several theories of history. One of these is what Rigby calls "productive forces determinism," the model that Marx summarized in the preface to *Critique of Political Economy*. According to this model, social institutions develop because they are appropriate to, and further the development of, productive forces. Many neo-Marxists consider this model inadequate and seek to explain Marx's statement away, but Rigby convincingly shows that productive forces determinism was an idea that Marx continued to use as an explanation of sociopolitical systems and historic change. Moreover, generations of Marxists accepted it as the orthodox interpretation, and a number of contemporary Marxists have elaborated and defended it.

In relentless step-by-step arguments, however, Rigby demonstrates that productive forces determinism is philosophically untenable and empirically false and that it is of no practical utility to the historian interested in the dynamics of historical change, the causes of revolution, or the classification of different social systems.

Rigby then subjects an alternative model of social processes, also derived from the writings of Engels and Marx, to similar critical analysis. This model is the dialectic of base and superstructure. Rigby correctly interprets the "base" to comprise all of those elements that make up relations of production, including ideas, attitudes, and predispositions. Whether a given feature of society belongs to the base or the superstructure, he argues cogently, depends on its function in this dialectic, so that, say, the property laws stated in the *Code Napoléon* obviously belong to both categories (this is my example, not Rigby's). Indeed, Rigby could have gone a step further by suggesting that the work ethic and the acquisitiveness of modern Western societies, according to which a person's worth is measured by his or her wealth, are essential elements of the base of capitalism.

After also subjecting this model to rigorous analysis and to testing against the historical evidence, Rigby concludes that the concept of the mode of production based on the primacy of production relations over productive forces is very useful for classifying social forms and for explaining long-term historical change. In the concluding chapters of the book, this interpretation is extended to a dissection of Marxist theories of the state and ideology. In the chapter on the state, I miss references to a statement made by Engels that the very existence of the state is a symptom of unbridgeable social cleavages and to Marx's discussions of bureaucracy. In Rigby's discussion of ruling-class ideology, I would have liked to have seen a reference to the notion that ruling classes are condemned to misread reality because a correct reading of reality would demonstrate to them their own impending doom. But, such minor criticisms notwithstanding, I am impressed by the wealth of information concerning many ages and cultures that Rigby adduces, by his thorough acquaintance with the writings of Marx and Engels, and by the wide knowledge of Marxist and Marxian literature that he displays. His book is the product of a razor-sharp mind that knows how to spot tautologies and other logical fallacies. Again and again he dips familiar phrases from interpreters of Marx into the acid of his analysis, and we watch them dissolve into nothing. This volume is an excellent contribution to historiography.

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ROMAN SZPORLUK. *Communism and Nationalism: Karl Marx versus Friedrich List*. New York: Oxford University Press. 1988. Pp. ix, 307. \$32.50.

Roman Szporluk's comparative study of Karl Marx and Friedrich List is nothing if not ambitious. Szporluk's goal is to redraw "the conventional map of the correlation of intellectual and political forces in nineteenth-century Europe" (p. 14). He means that the specter haunting capitalism during the nineteenth century was not only communism, which sought the elimination of intermediate loyalties such as the state and the integration of humankind as species being into a unified world, but also nationalism, which called for using the state to coordinate and protect the interests of the nation in a system of economically developed nation-states. Marxism is a critique of capitalism, but it also should be considered a confrontation with nationalism. In turn, nationalism is a political competitor of socialism, but it is also a particularizing analysis of capitalism. The relationships of nationalism and socialism to capitalism, therefore, are "triangular, not bipolar" (p. 14). In addition, Szporluk holds that nationalism is neither an intellectual nullity nor an anonymous discourse. One thinker at least, List, understood the nature of the confrontation between nationalism and capitalism better than did both Marx and other nationalists. As

Szporluk puts it, "There was no *single*, automatic nationalist response to the Industrial Revolution. Had there been one, individual thinkers would indeed not have mattered. But they did" (p. 99).

Basing his analysis of Marx on a recently discovered manuscript in which Marx attacks List and on Marx's critique of Hegel, Szporluk reads the *Communist Manifesto* as an antinationalist tract as well as a revolutionary document. But Marx was wrong in holding that capitalism was creating class solidarities that would eliminate partial loyalties, such as nationalism, that stood in the way of universal community. By reviewing the successes of nationalism during the rest of the nineteenth century, even among Marxists and communists, Szporluk shows the correctness of List's view that nationalism was closely linked with capitalism. In contrast to the German romantic nationalists, List accepted the power and inevitability of capitalism as much as he accepted such liberalizing tenets of the Enlightenment as constitutional government and the protection of civil liberties. What he did not accept was the universalizing doctrine of free trade. Much as Marx saw nationalism as the legitimizing ideology of bourgeois domination, List saw free trade as perpetuating the domination of developed economies over undeveloped ones.

Szporluk's fundamental points seem to me well taken—Marx did purposefully reject nationalism, thereby lessening the usefulness of his thought in analyzing the development of capitalism after his death; List did grasp nationalism's fundamental place in development; and List's nationalism was different from that of the German romantics. But in comparison to Marx, List's impact was ordinary rather than massive. List was an influential economist who had a number of followers in the late nineteenth century, but his thought was not rich enough to provoke an enormous interpretive literature, not inspiring enough to engage the imagination of revolutionaries, and not unsettling enough to produce political transformations. On the other hand, the successes of the newly industrializing countries of the Pacific Rim during the past twenty years seem to have come from pursuing a Listian policy of strong government, protection of property rights, protectionism, and nationalism. Their successes contrast with the failures of developmental efforts based on class-oriented economics. Szporluk may not have redrawn the intellectual map of the nineteenth century entirely, but he does seem to be right in pointing out that List grasped a fundamental relationship between capitalism and nationalism that Marx did not.

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DAVID I. KERTZER. *Ritual, Politics, and Power*. New Haven: Yale University Press. 1988. Pp. xi, 235. \$22.50.

Shortly before I joined the faculty of the University of California, Los Angeles (UCLA), in the 1950s, the student committee on traditions (now defunct) pro-

claimed a new UCLA tradition: no one was to walk over the great seal set in the floor of what was then the university library. Instant symbolism (soon abandoned) swelled the rich body of rituals that universities, like other institutions, accumulate over time. This book is predicated on the view that the importance of rituals and of symbolism in politics is generally underrated or ignored, and it is dedicated to demonstrating how all-pervasive rituals and symbols really are. Three hundred years after an English revolution provoked in part by questions of ritual, two hundred years after a French revolution that launched studies of revolutionary symbolism by the score, this notion seems a bit supererogatory.

Based on wide reading in anthropology, political science, and, sometimes, history, David I. Kertzer's study suggests how ritual is employed, deliberately or unconsciously, to affirm or reinforce power, legitimacy, group solidarity or national cohesion, in societies at all times, all over the world. This fact will not be news to most historians, but the encyclopedic coverage of the theme and the wealth of examples Kertzer provides make for interesting reading.

Many of Kertzer's anecdotes are fun, and some are stimulating, for example, when Louis-Philippe's political appropriation of Napoleon's remains is compared to an African king's expropriation of his predecessor's jawbone. Yet sustained interest is marred by the rolling barrage of argument designed to break down open doors; by intimations that ritual can provide exclusive explanations of situations that are, by nature, overdetermined; by too many quotations; and by pockets of jargon. Nuances get lost in the number of exemplifications. Studies in depth of particular societies and situations would better make the author's point.

Like the original premise, some of Kertzer's assumptions seem ill founded. I am not persuaded that "we could not get out of bed in the morning if we did not subscribe to the view . . . that the world simply presents itself in the form in which it is perceived" (p. 4). Seventeen pages of bibliography (and a good deal besides) testify to widespread awareness that "our Notions of reality are the product of an artificially constructed symbol system" (p. 4), though not of that alone. Received wisdom does not ignore the important role of symbol and ritual in the political process (p. 8). Organizational allegiance need not "be expressed only through symbolism" (p. 16). And do U.S. elections really "foster the illusion that American government is the result of the free, informed choice of the entire citizenry" (p. 49)? The author and I evidently read different newspapers.

The back cover of the jacket carries high praise from respectable colleagues. Those who need a refresher course in the obvious should not fail to buy this book.

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BONNIE S. ANDERSON and JUDITH P. ZINSSER. *A History of Their Own: Women in Europe from Prehistory to the Present*. Volume 1. New York: Harper and Row. 1988. Pp. xxiii, 591. \$27.50.

This is a book all historians of Europe (and of countries affected by European history) ought to read and to have on their shelves. It provides the missing half of what has been read and taught until now. Bonnie S. Anderson and Judith P. Zinsser have amassed, digested, and analyzed the vast amount of research that has occupied women's historians for more than two decades. Taking the initials of the authors' names, we might well call this volume the A to Z of European women's history.

The book is blessedly free from ascribing prehistoric male dominance over women to archaeological or biological evidence. Of all early written evidence, Anderson and Zinsser consider "warrior culture" the most convincing explanation for women's dependent and inferior position (p. 15).

The authors state clearly that they have (somewhat unwillingly) come to the conclusion that "gender has been the most important factor in shaping European women's lives. . . . [H]istorical era, class and nationality are significant, but these are outweighed by the similarities decreed by gender" (p. xv). Taking gender, then, as the linchpin of their arguments, Anderson and Zinsser range over the development of the lives of European women from prehistory to the present. To do so they have eschewed traditional periodization and chronology, and, following the precept of seeing history through women's eyes, they have created chapters dealing with women's place and function in society. That is not to say that they do not use traditional historical signposts, such as the Renaissance or the Industrial Revolution. Those concepts, however, are here offered as points of reference in the major discussion of such categories as "peasant women," "women within the Christian churches," or "women of the cities."

Thus, within those categories we follow in detail the lives and situation of women. We see their work in fields and homes, their labor in childbirth, their problems in marriage, their relationship to property and sexuality, their reasons for entering monastic establishments as children and as elderly wives or widows, their education as young girls, and their teaching of their own children. The evidence establishing the traditions forged in the ancient world and during the Middle Ages is culled from a variety of geographic areas encompassing the European continent and the British Isles.

Throughout this first of two volumes, which takes us from prehistory to the seventeenth century, we are constantly reminded that many of the practices established during those periods were still current in the late nineteenth century and, in some parts of Europe, are current today. The most important of those practices are the "double burden" of women's work and the

"double standard" acceptable in male and female sexual behavior. We are overwhelmed by the never-ending work of peasant women and by the "double burden" of women's physical work in home, field, and workshop and their care of children and responsibility for the survival of the family. The authors cite Ann Cornelisen's *Women of the Shadows* (1976), for example, to compare the similar experiences of Sicilian women of the late twentieth century with those of Sicilian women and others during the Middle Ages.

Anderson and Zinsser brilliantly expound the relentlessness of inherited traditions for women. They are less convincing in explaining that other vital component of history—the development of change. Perhaps that is due to what seems to be a fact of women's history. The weight of patriarchal authority has shaped women's lives so that political and economic changes and technological inventions have actually had less impact on women than has women's relationship with others, particularly with men. Anderson and Zinsser have certainly shown that in all classes of society gender has been a most important factor in shaping women's lives. Whether it has been the most important—whether, that is, it must share importance equally with class—remains open to debate.

There are two reasons why this book is so significant. First, it offers what is missing from traditional history books. Second, it brings home to us with a shocking impact what is wrong with those history books that do not include women. This book itself is only half of a history. It is the counterpart of what has previously been accepted as history—that is, the history of men. This book, however, does not pretend to be history writ large. It is defined clearly as "a history of their [women's] own." Even so, this history does leave one asking: What else was happening in the world? What were the men doing? Why did peasant women work so hard? Why did only women have a double burden? How did the wars that affected women so drastically come about? The book lacks the context of the overall picture. It no longer comes as a surprise that for centuries historians have excluded women. A reading of this volume brings a new revelation, nonetheless, of the vacuousness—indeed, falsity—of a history that covers only half of the people and their concerns.

SUSAN GROAG BELL
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GEORGE MODELSKI. *Long Cycles in World Politics*. Seattle: University of Washington Press. 1987. Pp. 244. \$30.00.

GEORGE MODELSKI and WILLIAM R. THOMPSON. *Seapower in Global Politics, 1494–1993*. Seattle: University of Washington Press. 1988. Pp. xii, 380. \$35.00.

George Modelski is a lifelong contributor to writings on the philosophy of history and international relations theory. This volume is the first in a trilogy and lays the foundations of the other two volumes. It sets forth the evidence, concepts, and framework for the idea of long

cycles and offers the main theoretical arguments for such an approach. Modelski defends what he describes as a new perspective on international politics along three lines of analysis: the historical, sociological, and relational. Historically, he seeks to demonstrate that global wars show patterns of recurrence at intervals of one hundred years in the modern state system. He offers a historical narrative that examines the long cycles process at work over the past five hundred years. During this period, four major powers have emerged in positions of leadership as a result of global wars: Portugal, the Netherlands, Britain, and the United States. Sociologically, Modelski draws on the conceptual frameworks of Talcott Parsons to construct what he calls a learning model to assist the understanding of his proposition that the long cycle is driven by the functional requirements of the global political system. Finally, he traces the relationships of long cycles with other major social processes and structures, including the growth of nation states, party systems and what he calls dependency and dependency reversal.

Throughout the volume, Modelski undertakes to connect with the sources that foreshadow his work in the literature of international relations: premodern works, including Thucydides and Polybius, and modern literature on the balance of power and sea power and international relations theory that he maintains had its beginnings with Quincy Wright's *A Study of War* (1942). Alongside theory, he applies the traditions of the social sciences (Parsons) and history not only in the postwar period but also in the experience of nations in organizing peace and order on the planet.

The second volume in the trilogy is *Seapower in Global Politics, 1494–1993* by Modelski and William R. Thompson. Through a time series of global powers for the five hundred years of the study, the authors seek to demonstrate that the high points in seapower concentration coincide with states achieving world power status in the long cycle of global politics. Whenever a state controlled more than 50 percent of the world's oceanic capital warships, it emerged as the paramount world power, whether it was Portugal, the Netherlands, Great Britain, or the United States (whereas Modelski focuses on global powers, Modelski and Thompson concentrate on world powers as they define them). The study is a massive research endeavor providing an inventory for each year beginning in 1494 of the world's major navies (Portugal, the Netherlands, Britain, the United States, France, Spain, Russia, and Germany) and regional powers (Venice, Italy, the Ottoman empire, and Japan). However much the form of the capital warship has changed, from the mighty ships of the age of discoveries to the aircraft carriers and missile submarines of the 1980s, its decisive role in world politics has remained the same. Seapower and the world's major navies have been the determinants of global politics in the oceanic age.

George and Sylvia Modelski continue work on the third volume in the trilogy. *Documenting World Leadership* is advertised as a collection of political documents

spanning five centuries that, through public statements, communicate the goals, aspirations, and policies of the nations that were the leaders in the global political system. This volume is intended to demonstrate how global policies reflect the superior seapower of leading nations in given historical periods.

If we turn from the content of the three volumes to criticism, three areas of commentary appear. First, Modelski, while offering a new perspective on international politics, is also at home in the traditional literature of international relations. Second, whereas many systems theorists eschew historical analysis, preferring that the purity of their theoretical constructs not be tarnished by concrete historical examples, Modelski in both theory and practice is immersed in historical analysis. Third, he confronts the most severe challenge to the integrity of his analysis when he moves from pointing up relationships to arguing that one set of factors determines the status of global powers in given historical periods and that seapower "determines" world power.

Modelski devotes a full chapter to the literature of international relations, but reflections on concepts and conclusions in the discipline appear throughout the book. He examines the literature on the balance of power, both classical and modern. He is, of course, especially conversant with the writings of Alfred Mahan and Bernard Brodie. He treats problems of historicism, on the one hand, and compares and contrasts the theoretical assumptions of his long cycles approach with Arnold Toynbee's analysis of world civilizations. He contributes a separate note on a "newly rediscovered field," "international political economy," as it has evolved from Immanuel Wallerstein to Robert Gilpin. My sole criticism is that his commentaries more often than not are "separate notes," which have little to say about other broad areas of international relations theory with which Modelski is familiar.

Modelski is far more of a historian than are most social science theorists. Indeed, these volumes are books about history, especially such chapters as "Narrative," "Nation-states," "Party Systems," and "Dependency" in *Long Cycles in World Politics* and all of the chapters in *Seapower in Global Politics*. Some historians may criticize his use of history and his rather adventurous employment of theoretical concepts and classificatory systems. But Modelski's approach can be distinguished from the writings of David Easton and Morton Kaplan precisely because he does not shrink from invoking concrete historical examples. In contrast, he is never far removed in his theorizing from "the patient in the bed," a quality that justifies reviewing his work in a historical journal.

The gravamen of criticism of Modelski's work is likely to center on the issue of determinism. From Marx to the present, the great debate about social science theory falls in the broad area of claims by some theorists that one set of factors or another determines a given pattern as opposed to the claims by others that there are important but indeterminate relationships

that manifest themselves. Much theorizing in the social sciences has suffered shipwreck on the shoals of this recurrent problem. Modelski seems at times perilously close to determinism, for example, when he equates the concentration of seapower, measured by numbers of capital warships, with global primacy in world politics. At other times, especially in chapters in *Long Cycles in World Politics*, he qualifies this view and seems fully aware of the contingent aspects of history that override any causal linkages that are too simple.

For historians and social theorists, the true test of Modelski's theorizing is likely to require the kind of scrutiny that comes with intensive seminar discussion. He describes the beginning of his study as occurring during a sabbatical at Harvard University in 1973. Dedication of *Long Cycles in World Politics* to C. A. W. Manning suggests a longer gestation period. Manning's own scholarly work never reached fruition, but he influenced a score of American scholars of international relations, including the late William T. R. Fox, who in turn taught several generations of scholars at Columbia University. Surely two decades and more of serious inquiry by a respected American scholar such as Modelski merits more than a premature judgment from a first reading on such a central question as determinism.

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JOHN H. PRYOR. *Geography, Technology, and War: Studies in the Maritime History of the Mediterranean, 649-1571*. (Past and Present Publications.) New York: Cambridge University Press. 1988. Pp. xviii, 238. \$39.50.

John H. Pryor's thesis, simply stated, is that too much emphasis has been placed on political and cultural considerations in the search for factors of causation and change in the *guerre de course*, the ever-present competition and frequent conflict among the Byzantine, Islamic, and Western maritime powers for control of Mediterranean shipping from the seventh through the sixteenth centuries. Instead, Pryor successfully argues that a combination of factors, particularly the "nexus between technology and geography" (p. 9), was by far more important than historians have previously assumed, that the corpus of primary evidence used thus far has been almost entirely Western European, and that it is insufficient to provide a complete picture of the inter- and intra-relationships at play in this historical drama.

Pryor adds much to the discussion, mostly by the point of view he applies to the already prevailing knowledge of the era. The first three chapters present a comprehensive and careful look at the limitations that geography, meteorology, ship design, and navigational factors placed on all of the peoples who sailed (or rowed) the Mediterranean from antiquity to the early modern period. The discussion and drawings of comparative ship design in each arena tend to deemphasize

its importance as a cause for political and economic change. Of particular interest is a discourse on how the effectiveness and range of warships (galleys) were limited in direct proportion to how much potable water they could carry, for example, a twenty-day supply allowed a cruising range of approximately 960 nautical miles based on an average cruising speed of two knots. Throughout these chapters a glossary would be welcome for navigational and engineering terms.

Chapters 4 and 5 cover the dynamics of the ninth through twelfth centuries, during which Islam, then Byzantium, and finally the Christian West gained control of the key ports and islands of the "trunk routes" along the northern shores of the Mediterranean. Pryor argues here that Islamic technology and strength were never inferior to the others, as some historians have suggested, but that "prevailing weather patterns" and "much greater distances" (p. 109) caused the loss of Muslim hegemony, at least so long as the main Islamic centers were in Egypt and North Africa. Chapter 6 documents the intentional bid for dominance over the commercial routes by the Christian West in the eleventh and twelfth centuries and how the revenue generated was essential to the survival of the Crusader states. Pryor makes a strong case, however, for the continued prosperity of Muslim and Byzantine trade, even though a handful of Western Christian maritime cities "controlled the lion's share" (p. 152).

The harassment and gradual displacement of the Western Christian and Byzantine trade by the Turks and Barbary corsairs during the thirteenth through sixteenth centuries comprise the final chapters. Pryor stresses that the balance of power in this process was not tipped by the well-known naval encounters, such as Lepanto, but by a persistent wearing down of the "tenacious resistance of Venice and the Hospitallers of Rhodes" (p. 181). All in all, it took the Ottomans 120 years to achieve their ends.

Pryor's concluding statements are weak, considering the scope and carefully argued material throughout, although generally the book is well written and a major addition to the scholarship. There is great need for a fold-out map to display the multiple sites mentioned in the text. The few smaller maps provided are not adequate for this purpose, although they are useful for elucidating specific points. There is a glaringly sexist remark on page 171. Otherwise there are few errors.

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PAUL GREENHALGH. *Ephemeral Vistas: The Expositions Universelles, Great Exhibitions, and World Fairs, 1851-1939*. (Studies in Imperialism.) Manchester: Manchester University Press; distributed by St. Martin's, New York. 1988. Pp. xii, 245. \$49.95.

Histories of universal exhibitions and world fairs have tended toward the celebratory; often published for the latest such event, they are usually long on enthusiasm

and short on scholarship. Paul Greenhalgh presents the mirror image of these histories in this book; writing in a condemnatory tone, Greenhalgh sees in these events the window dressing of imperialism, colonialism, racism, and worker exploitation. Unfortunately, the scholarship is so shoddy that the book's value becomes that of an unsubstantiated polemic with, however, some of the trappings of a serious study. Previous celebratory publications, such as John Allwood's *The Great Exhibitions* (1977), have at least been well illustrated. The Musée des arts décoratifs catalogue *Le Livre des expositions universelles, 1851-1989* (1983) is a compendium of both illustrations and contemporaneous responses, and Wolfgang Friebe's *Vom Kristallpalast zum Sonnenturm*, translated as *Buildings of World Exhibitions* (1985), which is limited to architecture and the layout of each exhibition, is a valuable reference book.

The historian of a subject so ambitious and complex (and so little explored) as international exhibitions, 1851-1939, would either have to do a vast amount of research in several languages or depend on secondary works. Greenhalgh has chosen dependence, although few reliable secondary accounts exist. In any case, he seems to have limited himself mostly to shows in England, France, and America and to sources in English. This skews his text in a peculiar way, so that the English male tourist, journalist, or government administrator becomes the unacknowledged "eye of God," an unintentional demonstration of English imperial attitudes. When he does use reliable sources, such as Richard Mandell's *Paris 1900: The Great World's Fair* (1967) or Richard Rydell's *Let's Go to the Fair: Visions of Empire at American International Expositions, 1876-1916* (1984), his study has their merits. Thoroughly unreliable sources, however, are cited just as often, so that the reader is hard pressed to separate fact from fiction or fantasy. Many of his "factual" statements are both undocumented and erroneous—everything from proper names to the exhibitions' funding, organization, dates, scope, even location and layout; where documentation is provided, no page numbers are given. As a result, the book is of dubious value, both perpetuating past errors and stockpiling new ones for unwary scholars.

The point of view throughout the book is the cynicism of late capitalism toward the optimistic hopes for industrial development and progress that characterized an earlier period. Not realizing that the Saint-Simonians were heavily involved in early shows in France (national shows began in 1798), Greenhalgh heaps scorn on the early expositions' stated goals of promoting peace, education, trade, and progress. Disillusionment with industrial progress did follow, although later in France than in England, but to project it backwards is to miss the dialectic of history that one would have expected to be very much in evidence in such a book.

Greenhalgh's main thesis, that the shows were vehicles for imperialism, is put forth in a chapter surveying and condemning "native village" displays. But major

nations also set up their own "native villages," for example, Swiss chalets, Japanese gardens, and American log cabins, each accompanied by "natives" going about their business for the entertainment and edification of the public; by not adequately defining the real differences between these types of display, Greenhalgh has chosen a polemical over a historical analysis of imperialism.

Greenhalgh's book is part of a phenomenon that might be called "the rush to survey," the intellectual equivalent of fast food. Perhaps we should see it as a phenomenon in intellectual history related to leveraged buyouts, of dubious value and achieved with heavy debt.

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DAN VAN DER VAT. *The Atlantic Campaign: World War II's Great Struggle at Sea*. (Edward Burlingame.) New York: Harper and Row. 1988. Pp. xvi, 424. \$25.00.

Winston Churchill termed the Battle of the Atlantic the dominating factor throughout World War II and spoke of the U-boat offensive as a mortal danger to Britain's life lines. It is easy to understand why after reading Dan van der Vat's account of the Atlantic campaign.

Van der Vat's aim is to present a panorama of the Battle of the Atlantic, rather than individual aspects, and incorporate a new dimension that historians must take into account. This is, of course, the Ultra intelligence, derived from the British success at reading German messages coded with the Enigma machine. Nevertheless, the author is something of a revisionist about Ultra, believing that its importance has been exaggerated. Van der Vat claims Ultra was not the only or even the most important factor in winning the Atlantic campaign. Allied success was based on a combination of factors, including the use of escort carriers, airborne radar, and long-range aircraft working with surface forces equipped with new and improved weapons. Historians can only applaud this rejection of single or simple explanations of complex events. The author's decision to include a full chapter on World War I and two chapters on the interwar period is also praiseworthy.

Operations by surface forces are not neglected, but inevitably the major story is the submarine war. Van der Vat includes the traditional villains, notably the unwillingness of Royal Air Force Bomber Command to provide the relatively few long-range Liberator aircraft that would have made all the difference and the reluctance of the American chief of naval operations, Admiral King, to institute convoys along the American coast. The fierce convoy battles of the climax of the submarine campaign in early spring 1943 are vividly described, and the author is anxious to remedy the

frequent neglect of the important role played by the Royal Canadian Navy. After the Allied victory in the convoy battles, the period from June 1943 to the end of the war is relegated to a single chapter. Van der Vat emphasizes that the last two years of the Atlantic campaign were basically a struggle of technologies, which the Germans "won hands down—but six months too late" (p. 340). The performance of the few new Type XXI and Type XXIII U-boats in the closing days of the war was indeed sobering, and the long-term threat posed by the advanced submarine was not really mastered, then or now.

This work is intended for the general reader, and the author proudly and even defiantly rejects the use of footnotes. Van der Vat claims that both he and his wife, Christine (whose research is acknowledged on the title page), have done extensive archival work in Britain, the United States, and Canada and in German sources through the vast microfilm holdings in the American and British archives. There is a two-page listing of archival sources and evidence of this work in the text. The German war diary of the commander of submarines, for example, is cited a number of times. Unfortunately, the author's scorn of the usual scholarly apparatus makes it impossible to judge how deeply or widely these original sources were used. The bibliography is limited to a little over one hundred titles; the works included are not always the best; and there are some conspicuous omissions. The most important is Michael Salewski's *Die deutsche Seekriegsleitung, 1935–1946* (1970–73). The author writes well and with great assurance, which is only to be expected from someone the dust jacket identifies as chief leader writer of *The Guardian*. Unfortunately, he makes a number of mistakes, particularly in the first three chapters, but they are mostly minor. This is not a scholarly volume, but, as a book for the general reader, it is successful. It is comprehensive and well balanced, and one need have no hesitation recommending it to students. Those in the Department of Defense who are concerned with the current maritime strategy of the U.S. Navy would also profit from careful reading.

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MARK GARRISON and ABBOT GLEASON, editors. *Shared Destiny: Fifty Years of Soviet-American Relations*. Boston: Beacon. 1987. Pp. xxxi, 167. \$8.95.

In this otherwise excellent collection of essays, there are some minor irritants. Abbott Gleason contends that the American-Russian relationship between 1780 and 1880 "was not very important for either side" (p. xv). John Quincy Adams would have disagreed when Russian actions proved in part the impetus for the Monroe Doctrine. And Hans Rogger's perceptive piece on how Russians have viewed America traditionally and vice versa would set a clearer picture of the fifty years of

American-Soviet relations if it came at the beginning of the book rather than at the end.

These criticisms are less important than the contribution this study makes to understanding the problems inherent in controlling excesses in the most pervasive and important international rivalry of the late twentieth century. Gleason states the problem perceptively when he concludes that false expectations and misinterpretation of motives and actions are dangerous, not merely for the U.S. and the USSR but for the world. The changes wrought by *perestroika* give added emphasis to his admonition.

In the concluding essay, Mark Garrison traces the degree to which modern technology, especially atomic weapons, has forced leaders and public alike in the super powers to become more aware of the existence and idiosyncrasies of one another based on a more realistic, as opposed to propagandistic, examination of Russian and American historical development.

In the intervening essays, the key word is "understanding" when evaluating the respective viewpoints and peculiarities of Russians and Americans as they struggled to come to grips with their importance to one another. George Kennan transports the reader like a time traveler to the beginning of the relationship where he lays bare the foundations of the historical misperceptions that plagued fifty years of American-Soviet relations.

Like Kennan, John Lewis Gaddis stresses the nature of the USSR's threat to other nations as based on a realistic assessment of power, not Marxist ideology. Gaddis also notes where ideology was set aside and useful arrangements made to enhance a peaceful, if tenuous, *détente*. He concludes, "[A]greements between great nations are no better than the interests that lie behind them; no government is going to allow vital interests to be circumscribed by ink" (p. 29).

Adam Ulam, with his usual clarity of assessment, presents a sound case for a more professional and careful evaluation of motives in the USSR's ploys and maneuvers in both the diplomatic and military context. He reminds the reader that one can be too chauvinistic and can lose sight of the fact that often Soviet policy may not be as antagonistic to American interests as the Americans believe and that failure to perceive this can distort policy.

Alexander Dallin notes how often Americans respond to Soviet initiatives and foreign policy stratagems by rushing in like a bull, bellowing petulantly when the opponent dodges the rushes and only then trying to figure out how to deal with the nation antagonized or maligned by the American response. His examples drawn from John Foster Dulles, Jimmy Carter, and Ronald Reagan are well taken.

Robert Dallek focuses on other and similar American deficiencies and admonishes the reader to examine the history of U.S. policy toward the Soviet Union with a clear view of why certain policies were based on ill-conceived beliefs regarding Russian abilities truly to effect American security. He reminds us that rhetoric is

not an acceptable substitute for policy. His point is that Soviet threats to U.S. interests were real, American overreaction to them quite unreal. Rogger stresses not only that Americans have misperceived Russians historically but also that misperception has been true of the Russians who have judged Americans through either utopian or xenophobic eyes.

Scholars might wish that they could force Americans of all persuasions to read this book and see the Russians as they must be seen as actors on a larger stage and that American leaders, especially, could assess where and why they have been right in their approaches to the Soviets and, in particular, where they have gone astray. One might wish the Soviet leadership would be similarly informed, although it seems of late this enlightenment may be happening more successfully in the USSR than in the United States.

EDWARD M. BENNETT

Washington State University

ANCIENT

FRANÇOIS HARTOG. *The Mirror of Herodotus: The Representation of the Other in the Writing of History*. Translated by JANET LLOYD. (The New Historicism: Studies in Cultural Poetics, number 5.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xxv, 386.

According to Herodotus (*Histories*, 1.82), after a military victory, the Spartans, who had always worn their hair short, grew it long, but the defeated Argives, who had been long-haired before, cut their hair short. A Libyan tribe, on the other hand, shaves one side of the head but allows the other side to grow long (4.191). Indeed, Claude Lévi-Strauss himself could not have forged a text more easily susceptible to structural analysis than the *Histories* of Herodotus. Not only did the Father of History perceive the world in terms of bipolar opposites, but his narrative also appears to confirm another structuralist tenet: when there is more than one version of an event, all versions are true and collectively assert the truth of some underlying myth. In telling of the war between the mighty Persian empire and the tiny Greek states, Herodotus presented not one extended narrative but many repetitions of the same story in which a barbarian despot leads his prosperous nation across a natural boundary to attack a more primitive people, always with disastrous results. Croesus attacked the Persians, Cyrus attacked the bloodthirsty Massagetae, Cambyses the Ethiopians, and, finally, Darius attacked the Scythians. So readers are well prepared when Xerxes, enervated by luxury and lured on by fate, meets defeat at the hands of the outnumbered but tough and brave Hellenes. Herodotus, in effect, imposed on history a synchronic myth in which several different nations in turn played the role of "noble savage."

Now comes the English translation of François Hartog's monograph published in 1980, which is the first book-length analysis of Herodotus to emerge from the

French structuralist school. The heart of Hartog's book is a lengthy and minute examination of Herodotus's Scythian narrative. Hartog analyzes this portion of the *Histories* both diachronically, as the account of the fate of Darius's doomed expedition against nomadic tribes who would not stand and fight, thus foreshadowing Xerxes' fate against Greece, and synchronically, for the many "mirror" analogies between Scythian and Greek customs. Hartog treats Herodotus's Scythian logos as an imaginary construct whose purpose is more to elucidate Hellas than to explain Scythia. Indeed, scholars have long viewed with skepticism the historicity of Darius's Scythian adventures, so Hartog's analysis is scarcely original in its broad outlines. He does make a number of fresh observations on individual passages, however. The nomadism of the Scythians, for example, and their continuing retreat before the advancing Persians resemble the Athenians' abandonment of their city and retreat behind the wooden walls of their ships. Hartog also shows that the Scythians, who took scalps and drank from the skulls of their enemies killed in battle, were both "other" than the Greeks and, in their fight for freedom, identical to them. Herodotus's mirror thus reflects two ways, both the truth and its opposite. Somehow one would have liked to see Hartog put more stress on this irony: *l'autre c'est moi!* Indeed, the concept, never clearly defined, of narrative as "mirror" appears in many different guises in the course of the book and becomes confused with other images. For example, Herodotus is also given "spectacles" (p. 289), presumably better to perceive his own "two-headed" (p. xviii) image through the "double" (p. 329) or "distorting" (p. 380) mirror of history. In short, despite many individual moments of inspiration, this is a disappointing book, a repetitious agglomeration of articles already published with new but often irrelevant material. The book cannot, therefore, be recommended to the nonspecialist. Even sympathetic readers will often be tempted to hurl this volume against the wall for its obfuscation of the obvious, its vagueness, and its repeated unanswered and unanswerable rhetorical questions. The translator struggles, often unsuccessfully, to convey the author's convoluted and playful style in which almost every paragraph contains half a dozen crucial words in inverted commas. French-speaking readers may find themselves better able to appreciate the author's self-indulgent obscurity in the original edition.

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PAUL PLASS. *Wit and the Writing of History: The Rhetoric of Historiography in Imperial Rome*. (Wisconsin Studies in Classics.) Madison: University of Wisconsin Press. 1988. Pp. x, 182. Cloth \$37.50, paper \$17.50.

In the thirty-two years since Sir Ronald Syme's *Tacitus* (1958) appeared, it has remained without a rival. But a number of more modest books have undertaken to

illuminate different aspects of the historian's craft. Paul Plass's slender volume examines Tacitus's use of wit and pointed language, *sententiae*, not just for stylistic virtuosity but also for political judgment.

Although the title of the book does not focus on Tacitus, he receives much the greatest part of the author's attention. Suetonius, Dio Cassius, and Seneca are also invoked to support Plass's thesis, but this book is essentially about Tacitus. And a good one it is.

Plass examines in the utmost detail the various devices of language and rhetoric, such as epigram and antithesis, that create an atmosphere of wit and "penetration into a political and moral reality that is often irrational if not idiotic" (p. 5). Many epigrams were originally political jokes that later passed into literature and were very effective because they were based on incongruity. Wit served to expose the moral and political absurdity to which the principate could lead; Tacitus employed it to devastating effect because he wished to unmask the political implications of moral irrationality. His mastery of epigram enables the reader to discern that often in politics there is less than meets the eye: the subject is empty, insubstantial, spurious. Violation of expectation, *para prosdokian*, to use the Greek expression, is crucial for shocking and convincing, as well as entertaining, the reader.

The result is that "Roman politics thus dies the death of a thousand stabs, ruthlessly punctured by each epigram or anecdote of negation cumulatively deflating coherence and integrity" (p. 126). With this careful analysis Plass gives strong support to the views of scholars such as Viktor Pöschl who see the irrational as one of Tacitus's major themes.

The book is well written but still slow going, because it is packed with material, and some of the philosophical and linguistic discussion is technical and difficult. The notes are full, the bibliography selective and instructive. Three errors of authors' names require correction, so that those interested can find them: Clarke for Clark, Klingner for Klinger, and Segal, C., for Segal, E. (Charles, not Erich). It is a pleasure to recommend this book to all students of early imperial historiography.

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A. J. WOODMAN. *Rhetoric in Classical Historiography: Four Studies*. Portland, Oreg.: Areopagitica or Croom Helm, London. 1988. Pp. xiii, 236. \$37.95.

In book 2 of the *De Oratore*, Cicero outlined what might be termed a rhetorical theory of historiography. He compared narrative history to the construction of a building: a "foundation" of impartially assembled information about the past, atop which a "structure" is erected through well-disposed narrative, convincing reconstruction of motives and effects, and fluid prose style. The historian uses such techniques to convey to readers not only what happened but also how and why

it happened, yet these techniques themselves are not specific to history but shared with oratory.

Cicero's remarks are of considerable interest even today, but they do not constitute, as A. J. Woodman argues (pp. 70–101), a complete theory of Greco-Roman historical writing. For one thing, Cicero said virtually nothing about historical genres, which have long been recognized as pivotal to classical thought about history. (Woodman does not cite Felix Jacoby's important discussion in *Atthis: The Local Chronicles of Ancient Athens* (1949), and his footnotes are short on references to Arnaldo Momigliano.) Nor is Woodman accurate in expounding Cicero's theory: the historian builds on fact; he does not adorn a "hard core" of fact with an "elaboration" of "plausible" fiction (Woodman misunderstands the rhetorical concept of *inventio*).

Woodman's chapter on Cicero is a centerpiece for his discussion of five classical historians: Herodotus and Thucydides (pp. 1–47), Sallust and Livy (pp. 117–46), and Tacitus (pp. 160–90). The three Romans especially were obviously much influenced by rhetoric; Woodman all but ignores Polybius, a much harder case for his proposition that "we perceive historical truth differently from the ancients" (p. 202). And, in fact, Woodman's application of the Ciceronian theory shows that it is too feeble to cast much light on classical historiography. For instance, Woodman infers from the numerous Sallustian echoes in Livy's preface that "Livy is writing history in the tradition of Sallust" (p. 131), while omitting comment on Livy's crucial rejection of Sallust's contemporary history in favor of a history of Rome from its origins.

Although there is some truth to Woodman's belief that classical and modern historical writing differ in kind, most of his specific arguments for this view are too exaggerated or impressionistic to make a convincing case. The chapter on Sallust and Livy draws a protracted distinction between Sallust's "Thucydidean style" (Cato the Elder's enormous influence is ignored) and Livy's subsequent adoption of a more periodic "Ciceronian" style; this distinction supposedly results from the difference between Sallust's "critical attitude" and Livy's "optimistic view of Roman history" (p. 146). But Livy's brooding preface, which Woodman argues was written during the troubled years before Actium (pp. 128–34), causes major difficulties for this interpretation. Does optimism necessarily correlate with prose style? Livy's "optimism" was, in any case, not an ordinary one.

Tacitus receives scarcely better treatment. From the preface to the *Histories*, Woodman writes, "it certainly cannot be assumed without qualification that Sallust is . . . Tacitus' main model" (p. 165); Tacitus's views "are in exact agreement with the recommendations for pleasurable historiography which were laid down by Cicero and Livy; yet 'it was only by embracing the alternative style of Sallust that he could portray himself fully as the historian of disenchantment'" (p. 168). Such contradictions (and there are many) suggest the fragil-

ity of Woodman's analysis and the impoverished character of his categories.

Woodman's central contention is that "classical historiography . . . is primarily a rhetorical genre and is to be classified (in modern terms) as literature rather than as history" (p. 197). This simplistic thesis, which has attracted some recent literary critics, now threatens to undermine scholarly grasp of the more serious side of classical historiography.

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A. B. BOSWORTH. *From Arrian to Alexander: Studies in Historical Interpretation*. New York: Clarendon Press of Oxford University Press. 1988. Pp. x, 225. \$57.00.

"The study of Alexander . . . is in large part the study of Arrian, who provides the constant thread against which the rest of the tradition must be assessed." Thus, the opening of chapter 2 describes the theme of this rich little book. Developed from a series of seminars given by A. B. Bosworth at Oxford, this monograph deals with Arrian of Nicomedeia, who, in the early second century, combined a career in the Roman imperial service with an outstanding literary effort. Arrian wrote the best surviving account of Alexander the Great.

Bosworth has emerged as one of the greatest living scholars of Alexander, perhaps among the best ever. He is completing a historical commentary on the Greek text of Arrian, a task—as users of the commentaries on Herodotus, Thucydides, and Polybius know—that is among the most difficult that historical scholarship offers.

The present volume derives from the nearly completed commentary. Its two parts comprise an examination of Arrian's career and methods and an analysis heavily dependent on Arrian's account of several historical problems in Alexander's life. Bosworth's second chapter must now be regarded as the pocket essay on Arrian's life and literary output. He has built on the work of Philip Stadter, P. A. Brunt and others and has produced a portrait of Arrian that gives more emphasis to the high quality of his literary career than previous scholars have.

Prose literature in antiquity, including the writing of history, often included rhetorical techniques to provide special insights into character or to delineate issues through speeches and letters. Many of these techniques involved literary invention, and the enduring problem of the modern reader is to understand the nature of these rhetorical materials and the extent to which they may represent a distortion of what actually happened.

Bosworth shows that Arrian actively employed such rhetorical devices as part of his literary technique, and this recognition establishes an effective method of historical criticism when using Arrian's works: separate the original material from the secondary embellish-

ment (whether this embellishment was in Arrian's sources or was his own invention). Some incidents in the later books of Arrian provide Bosworth with the opportunity to apply the method. The results are several new interpretations of events in Alexander's career and insights into Arrian's techniques.

Students of the Macedonian conqueror will thus be interested in Bosworth's suggestions that a Roman embassy did approach Alexander toward the end of the king's life, that Arrian's account of Alexander's reconciliation speech at Opis in 324 B.C. was a rhetorical invention, that the truth behind the reports concerning the cause(s) of Alexander's death is probably beyond recovery, and that the last testament of Alexander describing his plans for the future is reliably described. Some Arrian fundamentalists may be aghast at Bosworth's views, but most serious scholars of Alexander will benefit from this important book.

My only complaint is directed at the publisher. In an age in which computer-oriented printing technology often reduces the costs of publication, Oxford University Press should be able to produce a book of two hundred twenty-five pages—even one that necessarily includes many Greek characters—for less than this one costs.

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TJEERD H. VAN ANDEL and CURTIS RUNNELS. *Beyond the Acropolis: A Rural Greek Past*. Stanford: Stanford University Press. 1987. Pp. xii, 221. \$27.50.

As a general rule, reference books are known for the essential information they contain rather than for the lively reading they provide. It is a rare and happy situation when a "useful" book is also a pleasure to read. Such is the case with this book by Tjeerd H. van Anandel and Curtis Runnels, a survey of fifty thousand years of history in the southern Argolid. It should be required reading for every person with a serious interest in any period of Greek history.

The subject is not promising: the book is a summary of the results—to be published in full detail later—of an intensive archaeological survey of the southern Argolid, a small region that the authors characterize as "an insignificant part of Greece" (p. 169). The purpose of the survey was to find sites, not artifacts, and, of the artifacts incidentally discovered, the authors admit that "rarely can so much time have been spent by so many on so visually unattractive a set of materials" (p. 39). This story is not of man-made objects but of the distribution and function of sites and settlements from the time of the first human occupation in the region. The authors are especially concerned with changes over time and with the interaction between human occupation and the natural environment. That is hardly gripping material.

The value of the account stems from both the significance of the evidence and the manner in which

the story is told. During the past two decades, the worth of survey archaeology has become increasingly clear. Rather than concentrating on one site, surveys embrace larger regional contexts in a diachronic perspective. At the most general level, this book illustrates the goals and fruits of intensive survey. In the account, which is far more than a rural economic history, the authors describe human use of the region in five major periods from the caves of the early Neolithic period to the beach hotels of the present. In the final chapters, they summarize the two parameters of the book: first, conditions of the land and sea and, second, the anonymous people who alternately cared for and raised havoc with the environment of the southern Argolid. The authors argue convincingly for abiding patterns of nucleation and dispersal according to the region's isolation from or connection with the larger Greek world.

One of the book's great strengths is the way the story of this sleepy region is interwoven with developments in more important areas, as the authors isolate what is typical and what is exceptional about the region's development. Evidence from the southern Argolid thereby can shed light on significant questions pertaining to all of Greece. The survey, for example, revealed no traces of occupation in the early Dark Age until ca. 850 B.C. It would seem, consequently, that Vincent Desborough's picture of a Greece that was "empty of life" was not as farfetched as some scholars believed.

The presentation of the story is engaging, leavened throughout with wit and common sense. The authors point to problems of interpretation—the discussion of population estimates is particularly cautious—and readily admit that their findings should be regarded as preliminary. Still, the account is never overly tentative. In fact, the authors challenge several received positions, such as Colin Renfrew's view of the importance of olive culture in the early Bronze Age and Claudio Vita-Finzi's conclusion that there were two periods of soil deposit in the Mediterranean. (The authors believe that the olive became important in the later Bronze Age, and they see evidence of seven alluvial deposits.)

The gracefully written text is accompanied by numerous maps and superb illustrations. The lengthy commentary accompanying each illustration is actually a synopsis of data that reinforce major findings. The findings are of major significance. I hope that an inexpensive, paperback version of the book is forthcoming so that this excellent case study of method and of Greek country life can be as widely appreciated as it deserves to be.

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GLENN RICHARD BUGH. *The Horsemen of Athens*. Princeton: Princeton University Press, for the Center for Hellenic Studies. 1988. Pp. xvii, 271. \$32.50.

This book, the first comprehensive treatment of the Athenian cavalry in over a century, will replace M. A. Martin's *Les Cavaliers athéniens* (1886) as the standard work on the subject of the cavalry as a public institution. Although he does not ignore the archaic and Hellenistic periods, Glenn Richard Bugh emphasizes the fifth and fourth centuries B.C. for which plentiful literary sources allow the construction of a coherent narrative of events. Bugh's treatment of literary sources is judicious. Problems of *Quellenkritik* are taken seriously but do not predominate, and the author takes clear stands on cruxes. Bugh is a good epigrapher and squeezes much useful demographic information from the relevant inscriptions. There are also stimulating discussions of vase painting and sculpture; Bugh demonstrates a sensitivity to nuance and genre often absent in treatments of artistic material by institutional historians.

Bugh argues that the Athenian cavalry was originally a small force (approximately one hundred men) that patrolled Attica's borders and coasts against enemy raiders. He suggests an ideological explanation for Athens's failure to field a more respectable cavalry before the mid-fifth century: despite the financial expenses associated with horse raising, fighting from horseback was not regarded as especially noble because of the tradition of the hoplite infantry. One might pursue this line of argument further: leisure-class Athenians were expected (later, even required) to serve in a military branch that never achieved symbolic parity with the infantry (dominated by laboring citizens) or even (after Salamis) with the navy (dominated by the poorest citizens). The initial discontinuity between social wealth and status and military citizen status seems to have embedded an enduring structural ambiguity within the cavalry as an institution. Perhaps skirmishing in frontier zones with hit-and-run raiders (rather than fighting in close formation in the open plains, as did the hoplites) reinforced the paradoxically liminal position of the wealthy horsemen. This paradox, if fully worked out, might provide a sociopolitical context for Bugh's institutional narrative.

By the mid-fifth century, the Athenians had increased the size of the cavalry; by 431 B.C., the force numbered twelve hundred (including two hundred mounted archers). Bugh argues persuasively that the horsemen were mostly young (in their twenties and thirties), from well-off families. But, because there were not enough Athenians able to afford the full-time commitment that cavalry service demanded, state financial support was offered in the form of a maintenance allowance (*sitos*) and a subsidized loan for the purchase of a horse (*katastasis*). This larger cavalry saw extensive service in the Peloponnesian War, fighting without hoplite support in Attica to keep the Peloponnesian invaders from threatening the walled city. Meanwhile, Athenian cavalry units served in other theaters as well: Bugh details operations from the Peloponnesus to northern Greece, from Sicily to Asia

Minor. Ironically, this period of genuinely heroic action concluded with the horsemen's darkest hour: their unanimous support of the bloody puppet oligarchy of The Thirty (404 B.C.). The Athenian citizenry could not forget this episode after the democratic restoration. In the fourth century, the wealthy cavalrymen served under a cloud. The oligarchic interlude highlights the contradictions between the cavalry as a manifestation of a socioeconomic class with anti-democratic tendencies and its vital, but devalued, role as a branch of the democratic state's military forces. In the Hellenistic period, Athens's cavalry was reduced in size and reverted to its archaic role as a territorial defense force, a role it had never completely abandoned. Prosopographical considerations suggest that cavalrymen now often served well into middle age and that most were from the traditional horse-raising upper-class elite.

Inevitably, in a study of this scope, questions are raised that space constraints or the author's perspective leave unresolved. Some readers will regret the lack of fuller discussion of strategic and tactical issues: just how did the cavalry fight? The answer might shed light on the historiographic question of why cavalry actions are included or deleted from ancient accounts (pp. 126, 137). And what of the cavalry in civic ritual? A discussion of the role of the horsemen in, for example, the Panathenaic Festival might suggest how the ambiguous terrain between private distinction and public service was defined. Bugh's fine study provides a solid foundation on which we can hope much work will be grounded.

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ELIAS J. BICKERMAN. *The Jews in the Greek Age*. Cambridge: Harvard University Press. 1988. Pp. xii, 338. \$30.00.

If Anna's employer, the king of Siam, had read this book, he would have pronounced it a "puzzlement." How, he would have asked, could so eminent a scholar turn out such poor work?

The answer seems to lie in part in the transformations the work underwent. In the late 1950s, the chancellor of the Jewish Theological Seminary of America asked Elias J. Bickerman to write one volume of a series on Jewish history. Bickerman completed his work in 1963, only to find the project cancelled. Fifteen years later he decided to make radical revisions in his manuscript, evidently in the direction of making it a work of *haute vulgarisation*. He suppressed all of the voluminous notes of the 1963 version, and he employed a terminology calculated to appeal to a Jewish readership. Palestine is the Promised Land, the Old Testament the Word of God, and the Jews the Chosen People (the capitals are Bickerman's). The Greeks are pagans, their statues idols, and their religion heathenism. Bickerman had the right to turn out whatever kind

of book he wished, but, with pejorative vocabulary like that, he ought not to have called it "universal history" (p. ix).

In 1981, alas, Bickerman died with his manuscript still not ready to publish, so that it was later edited by the seminary's research staff. Their work should have been done more rigorously. On page 251, there is a paragraph that does not make sense either historically or grammatically. The bibliography lists a considerable number of works that Bickerman could not have used because they appeared after his death.

Bickerman treats the theme of stability and change in Jewish society from the last century of the Persian domination and the age of Alexander to 175 B.C.E. He omits the century between the revolt of the Maccabees and the appearance of Pompey and his Roman army. The most interesting section takes up topics such as the temple at Jerusalem and its services, priests and Levites, the Midrash, and, above all, the new forms of literature and new values that were creeping into Judaism as the result of the ever-closer relations growing up between Jews and Hellenes.

Unfortunately, the treatment has serious flaws. Not much effort is made to relate the changes in Judaism to the regimes that controlled Palestine. Did, for example, the relatively benign government of Antiochus III lead more Jews toward Hellenism than the exploitative policies of the Ptolemies? Little is said of the attitudes of the Jewish notables toward either Greek kingdom, although the information exists. The book is also studded with mistakes, as though Bickerman relied as much on a faulty memory as on carefully prepared notes. Indeed, he took the notes for the revision of 1978 on the backs of envelopes. Some of his mistakes seem to be slips of memory, for example, placing the death of Democritus exactly one century too early (p. 103). Others are howlers as a result of ignorance, for example, the assertion (p. 176) that, because of climate, olive trees would not grow in Egypt. That would have surprised Apollonius the Dioecetes (*P. Cairo Zen.* 59072; M. I. Rostovtzeff, *The Social and Economic History of the Hellenistic World* [1941] 1.344; and so on). Some very serious mistakes show Bickerman more at home discussing Judaism than Hellenism. He says that the civilizations of the Near East were "bookish," which seems to mean that they were based on written religious documents, like the Persian Avesta and Jewish Torah. Greek civilization, on the other hand, he says, was based on the spoken word (pp. 103, 180). That is just the reverse of how contemporaries saw things. Apion, the man who wrote the anti-Jewish tract that Josephus answered, reproached the Jews for being unlearned with only twenty-two books to their credit, whereas educated Hellenes possessed tens of thousands (Josephus, *Contra Apionem* 1.6, 15, 38).

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PAULA FREDRIKSEN. *From Jesus to Christ: The Origins of the New Testament Images of Jesus*. New Haven: Yale University Press. 1988. Pp. xii, 256. \$22.50.

Both the title and subtitle of this book suggest that it contains a discussion of the origins of Christology, that is, how Jesus of Nazareth came to be confessed as the Christ (or Messiah) and how he came to be portrayed by various New Testament writers. By and large that is what Paula Fredriksen discusses, but she also takes up related issues.

The book is arranged in three main parts. The first, containing the first four chapters, is descriptive. In it Fredriksen sketches the images of Jesus in the four gospels, Acts, and the writings of Paul within the context of the Hellenistic world of the Greek texts. The second part (chapters 5–7) is historical. Here she discusses the context of Jesus' life and ministry (ancient Judaism) and attempts to recover the historical Jesus. The third part (chapters 8–10) is explanatory. Fredriksen gives an account of the development and diversity of the images of Jesus in the proclamation of earliest Christianity and in Paul, the four gospels, and Acts. The three-fold approach to the topic is quite successful. Its disadvantages are that there is some repetition and that the reader looking for the author's total treatment of a Christological portrait has to look in two places; for example, John's Christology is treated in both chapter 3 and chapter 9.

The most provocative chapters are those that discuss the historical Jesus (chapters 6 and 7) and earliest Christianity (chapter 8). In regard to Jesus, Fredriksen applies rigorous, yet sympathetic, historical criticism to the canonical gospels. The Jesus that emerges is a Galilean charismatic, an exorcist, and an authoritative proclaimer of a future, apocalyptic Kingdom of God. He gathered disciples and was misunderstood by the Roman governor, Pontius Pilate, as a seditious figure (King of the Jews) and a messianic pretender. In his ministry Jesus attracted "Palestinian Jews from various walks of life—the poor and the well-off, tax collectors and zealots, prostitutes and Pharisees" (p. 128). Fredriksen asserts that the events surrounding and following Jesus' entry into Jerusalem (above all, the so-called cleansing of the Temple, signifying its destruction) explain Jesus' arrest and execution as a messianic pretender.

But how does one explain the rise of the Christian belief that Jesus' death and resurrection had saving significance for humanity? Fredriksen's proposal is that its roots "may lie in Jesus' remembered relationship with sinners" (p. 137). The repentance and faith of sinners ensured for them a place in the Kingdom of God; their commitment to Jesus replaced an atonement offering at the temple. After his death and resurrection, Jesus himself was interpreted as "the sinner's atonement sacrifice" (p. 138). The suggestion is new and worth considering. One of its limitations, however, is that it presupposes a regard for the temple

and its cultus among those with whom Jesus consorted in Galilee and elsewhere that may not have existed.

Fredriksen gives some attention to Christological titles (Son of Man, Lord, Son of God) and a great deal of attention to issues concerning relationships between Christians (predominantly Jewish) and the larger Jewish community. Fredriksen carries on a detailed discussion of early Jewish persecution of Christians and rejects various theories. She concludes that Christians were persecuted for proclaiming the coming of the Kingdom of God. That proclamation sounded like sedition (by "messianists from Palestine" [p. 155]) that could endanger Jewish communities, so a response was called for. The response consisted primarily of thirty-nine lashes as punishment.

As indicated earlier, the actual texts Fredriksen deals with in her delineation of New Testament images are the four gospels, Acts, and the letters of Paul. She does not take up other texts, such as the Deutero-Paulines, Hebrews, the Catholic Epistles, and Revelation. If she had, a very large book would have resulted. Fredriksen's book serves the general reader as an introduction to basic issues in the large and ever-growing field of Christology, and that is its main purpose. But in her book Fredriksen probes issues with skill and insight, giving a new twist here and there, from which specialists also can profit.

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MICHAEL GRANT and RACHEL KITZINGER, editors. *Civilization of the Ancient Mediterranean: Greece and Rome*. In three volumes. New York: Charles Scribner's Sons. 1988. Pp. xxvii, 720; xiv, 721–1298; xiv, 1,299–1,960. 1980. \$225.00 the set.

This impressive collection of ninety-seven essays is a mini-social encyclopedia, along historical lines, of the ancient Greeks and Romans. Its appearance is a major event in classical studies, where there are very few comprehensive reference works in English and none of this size and scope. Considering the rarity of projects like this, its place as a standard item on reference shelves is assured for a number of years to come and most deservedly so in this instance. Classicists and ancient historians will be delighted to have such a resource to send their students to and will themselves profit from the distilled learning of their colleagues. It will be an especially valuable reference tool for scholars in other fields.

The eighty-eight contributors are scholars of the first rank; most of them are recognized as authorities on their topics. The essays range in quality from good to exceptionally good; a fair number are gems of concise scholarship and learning. Those who consult an article in this collection are guaranteed a knowledgeable, thoughtful survey incorporating the most recent discoveries and theories and, in most cases, the latest trends in analytical method.

Nonclassicists may be surprised to learn that the past twenty or thirty years have witnessed an impressive increase in new evidence about ancient Greece and Rome and something of a revolution in methodological approaches. All branches of ancient studies have been deeply influenced by the new intellectual currents emanating from cultural anthropology, the "new archaeology," and the "new cultural history." These concerns are reflected not only in the various articles themselves but also in the plan and design of the project as a whole.

The work is arranged topically and comparatively. Greece and Rome are juxtaposed in pairs of essays within major headings—thus, "Greek law" and "Roman law" appear under "Government and Society"—or, where appropriate, subjects are combined in single essays ("Slavery"; "Interstate Relations"), which discuss first Greece and then Rome. Given the relatively restricted space allotted to many subjects (articles range in length from five thousand to thirty thousand words), description and analysis are necessarily done in broad strokes, but, because of the contributors' easy familiarity with the evidence and the scholarship, the result is usually a judicious and authoritative compromise between information and process. Readers will be especially thankful for the comprehensive bibliographies of ancient sources (in translation) and modern studies (mostly in English) appended to each essay.

Some topics are treated synchronically. Most of the essays, however, present their subjects historically. The chronological limits imposed by the editors are roughly 1000 B.C. to A.D. 500; however, the time spans covered by individual essays vary according to subject and the condition of the evidence. The essays on Greece mainly concentrate on the eighth century B.C. to the first century A.D., the Roman entries on the fifth century B.C. to the third century A.D., the periods of most interest to most classicists as well as the ones that are best documented. Two unavoidable consequences of this scheme of individual historical summaries, arranged in opposing pairs according to topic, are considerable overlap and repetition of material, on the one hand, and numerous contradictions and differing evaluations of the evidence, on the other.

Although the articles are self-contained and are meant to be consulted separately, the work as a whole is also designed as if the separate major headings were the consecutive chapters of a unified survey. The work begins with the section "History," separate historical summaries of Greece and Rome, and ends with an epilogue, a short essay on the history of classical scholarship from the Hellenistic period on (unfortunately, without a bibliography). The interests of unity and comprehensiveness would have been better served if the editors had included here an article on the Mycenaean civilization, as background for the many references to Late Bronze Age culture scattered throughout the essays, and a separate analysis of the sources, evidence, and methods (especially the new methods) of classical scholarship.

The twelve sections that make up the body of the encyclopedia roughly follow the logic of infrastructure, structure, and superstructure. Sections titled "Land and Sea," "Population," "Agriculture and Food," and "Technology" cover the ecology and the material and technological bases of the Greek and Roman cultures. Next are political and social institutions and behavior, under "Government and Society," "Economics," and "Religion." Two sections on *Privatleben* follow: "Private and Social Life" and "Women and Family Life." These twenty essays (over three hundred pages), together with the section on religion, constitute a unique summary of what is currently known about the lifestyles, leisure activities, interior lives, and psychology of the Greeks and Romans. The final grouping is devoted to intellectual and aesthetic activity: "Literary and Performing Arts," "Philosophy," and "The Visual Arts."

The aim was to cover both cultures as comprehensively as possible, and, except for the regrettable omission of the Mycenaean era, the collection gives a gratifyingly total picture. All of the major components, and most of the minor ones as well, of Greek and Roman civilization are given their own discussions. The influence of the "new cultural history" is evident in the choice of topics, which emphasizes the realia and the details of everyday life. Thus, under "Technology" we find not only the typical subjects of crafts, engineering, building techniques, and transportation but also essays on calendars and time-telling and book production. The essay "Alphabets and Writing" devotes considerable space to the social and psychological effects of writing in an essentially oral culture. Other examples of this sort are essays on foods and their preparation, on perceptions of an afterlife, attitudes toward sex, and prostitution. Especially gratifying is the attention given to women and family life: four essays are on women and marriage and one on birth control, childbirth, and early childhood (all by women, incidentally).

On the other hand, literature, philosophy, and the arts, normally great consumers of space, are scaled back (religion gets 25 percent more space than the whole of Greco-Roman literature from epic poetry to letter writing). This reduction to scale of the great cultural achievements of Greco-Roman civilization accents the intent of the work. The coverage of those subjects is complete but proportioned to their place as aspects of the total cultures. One might contrast this approach to the more standard plan of the new *Oxford History of the Classical World* (1986), which gives most of its space to literary, intellectual, and artistic matters.

It is precisely the sociological and, in the broader sense, comparative perspective of the collection that will most appeal to students of cultural history. To cite a few more examples, there are two essays devoted to class and class relations and a short but penetrating analysis of the institution of slavery and semi-slavery in classical antiquity. The recent keen interest in men's associations and their social and political functions, a staple topic in anthropological studies, is reflected in two long essays on symposia, dinner parties, and clubs

in Greece and Rome. I also single out for mention in this regard an essay on the strange (to us) practice of ruler worship and two essays on Judaism and Christianity, both written from the perspective of their historical relationship to the Greco-Roman culture. This separate treatment of subjects usually given only passing mention (if they are not left out entirely) in general surveys is the most noteworthy achievement of the collection.

As the selection of topics reveals, the project is very much a reflection of current fashions in historiography, focusing on those aspects of classical antiquity that are conducive to a history from the ground up: environment, subsistence, technology, women and marriage, ritual and ceremony, mental processes. Even within the framework of the traditional preoccupations of general surveys—government, politics, and war—the emphasis is away from individual events and personalities toward a broader social-historical treatment, concentrating on movements, conflict, integration, and ideology.

There are some editorial faults, however, that reduce the collection's efficiency as a reference work for a general audience. One is the paucity and placement of illustrations. The technological and art historical essays are fairly amply supplied with drawings and photographs, but none of the others are. This means, to cite a single example, that there is no drawing of a ship, although seafaring and naval warfare figure prominently in the essays on warfare and transportation and the article on transportation contains technical descriptions of ship design and construction. The same is true of the essays on book production, athletics, spectacles, and many other articles, where even a single illustration would have immeasurably enhanced the text. And it is inexcusable that there are almost no references to illustrations that do appear in the text. For example, there are five detailed drawings of ballistic machines in the article on engineering, but there is no reference to these in the account of siege warfare (pp. 694–96) nor to the excellent aerial photograph of a Greek theater that appears in the essay on Greek architecture (p. 1667) in the discussion of staging in the essay "Drama."

In fact, throughout the text there are only a few, sporadic cross references to other articles. This is a serious omission in a collection of narrowly topical essays. Most unsatisfactory, despite a length of 128 pages, is the index. I collected dozens of omissions, many of them serious. Here is a sampling. If one looks up "Population," one finds only "Population, Roman," under which there are references to but a single essay, "Roman Trade, Industry, and Labor." Again there is only "Roads, Roman" and, under "construction of," no reference to the technical description of Roman road building in the essay "Transportation" (nor, as already noted, are there cross references in the text). Similarly, there is only "House construction, Roman" in the index without reference to the article "Houses," which contains full descriptions of Greek and Roman house architecture. The unevenness in referencing and in-

dexing is particularly disturbing, since it limits the effectiveness of the collection as a reference work in the narrow sense.

Aside from these shortcomings, however, this ambitious project succeeds admirably; the editors and publisher are to be congratulated for planning and bringing to fruition so coherent and comprehensive a collection. In my opinion, it is easily the best general reference work on classical antiquity currently available, in English or any other language.

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SUZANNE DIXON. *The Roman Mother*. Norman: University of Oklahoma Press. 1988. Pp. 286. \$29.50.

That motherhood is a cultural construct that changed over time will come as no surprise to historians of the family. According to Suzanne Dixon, the chief function of the upper-class Roman mother was to inculcate traditional morality into her offspring rather than to supply personal, affectionate care. Dixon's view provides support for theories advanced by Lawrence Stone, Lloyd deMause, and others that the emotional affect among members of families in premodern Europe was low. Dixon discusses the demographic realities, including high rates of infant and juvenile mortality, that might have caused parents to avoid emotional investment in young children but does not speculate on the psychological effects of such practices.

Dixon's book evolved from a doctoral thesis at the Australian National University supervised by Beryl Rawson, an authority on Roman children. Both Dixon and Rawson, like Rawson's mentor Lily Ross Taylor, exploit conventional primary sources on private life, but, in addition, all assign special priority to inscriptions from the city of Rome. Thus, although Dixon focuses on the elite mother, she often draws comparisons with the lower classes by citing data on familial relations supplied by epitaphs of slaves, freedmen, and freedwomen. Dixon has sought to attract readers who are not specialists by drawing comparisons between the Roman case and modern Western ideals and realities of motherhood. Her book may be assigned to students: all Latin is translated, and chapters end with conclusions. The book, however, would have benefited from careful copy editing. There are misspellings, and the index lists nonexistent pages with Roman numerals.

Dixon finds little difference between the roles of *materfamilias* and *paterfamilias*: both parents were primarily disciplinarians and figures of authority. But, by virtue of *patria potestas*, the father had the power of life and death over his offspring, including unemancipated adults, and some fathers exercised that right. Exposure was employed as a method of family limitation; the father decided whether to raise an infant. Even elite women such as Tullia and Livia, who were pregnant at the time of divorce, surrendered their

newborns to the family of the baby's father. (Tullia died subsequently from the complications of childbirth.) Since all the authors of the primary sources on Roman motherhood were male, the sentiments of women are difficult to determine. If motherhood was benign and prestigious, as Dixon maintains, why was the birthrate among the upper class so low? Did Roman women, like Spartans, refuse to bear children? Some readily available, effective methods of both contraception and abortion were described in the popular literature and, no doubt, in oral tradition. Although Roman law generally viewed a child as the father's property, no court could prevent a woman from choosing to use such methods of family planning. A profusion of anecdotes showing that Roman children respected their mothers should not cause the historian to forget that patriarchy means "the rule of the father."

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JOHN E. STAMBAUGH. *The Ancient Roman City*. (Ancient Society and History.) Baltimore, Md.: Johns Hopkins University Press. 1988. Pp. xviii, 395. Cloth \$30.00, paper \$12.95.

The subject of this book is Rome's "physical and social environment" (p. xv). John E. Stambaugh gives "a concise survey of the city . . . throughout antiquity, testing the hypothesis that as art and literature provide evidence for the environment, the environment makes the art and literature comprehensible in new ways" (p. xv). He begins with two simple, but immense, questions (p. 2)—"What was life really like in the ancient Roman city? How did it look, feel, sound, smell?"—and to answer them pursues an ambitious strategy, drawing widely, and boldly, from the historical, literary, and archaeological reserves of classical scholarship. The result is a fascinating, but at times curious, piece of urban history.

The book has three parts. In the first, Stambaugh outlines Rome's chronological and topographical history to show how the city's physical appearance became more and more monumental as its political power increased. In the second, he presents a social profile of the city, treating such themes as population, government, services, commerce, housing, religion, and leisure. In the third, he appends chapters on Cosa, Pompeii, Ostia, Arelate, and Thamugadi to demonstrate how the pattern of Roman urbanism spread through parts of Italy and the West.

Each part provokes unease: the first because it is full of "potted" political and literary history redolent of the handbook; the third because it affords only a glimpse of its several subjects; and the second, as I will illustrate, because it smacks of the Merrie England style of social history once encouraged in James Dixon. Thus, Stambaugh is well aware of the abject poverty and

squalor in which most of Rome's inhabitants lived (he refers, for example, to their "shanties" [p. 43] and "ramshackle tenements" [p. 51]), and he is aware, too, of such distressing urban conditions as cheap construction and overcrowding, disease and poor medical facilities, food shortages and inadequate sanitation (he brings out the colorful details of slop buckets being emptied from upper casements and bodies being left in the streets to rot). But his sympathy for the city is so strong, his belief in Rome's ever-rising standard of living so resolute, that the true significance of these factors for properly determining the quality of life (or rather the lack of quality) enjoyed by the majority of Rome's population, at every stage of its history, is obscured. Continually impressed by the prestige building of Roman aristocrats—their basilicas and porticoes, aqueducts and arches, temples and theaters—Stambaugh avoids looking too closely into the *insulae* and *tabernae* of the poor, preferring instead to emphasize what he terms "the grandeur of it all" (p. 225). So a lack of historical balance is apparent and attributable, I think, to Stambaugh's eclectic methodology. As an alternative, a comparative paradigm of the preindustrial city might well have been instructive.

But it would be a mistake to deny the book's usefulness on the physical evidence. Stambaugh knows the monuments very well, and, with a keen sense of how they would have appeared to the ancient visitor, he can conjure up images of Rome that are memorable indeed. Thus, he is excellent when describing the changing aspects of the Forum Romanum or when bringing to life the Campus Martius of the age of Augustus. The text, moreover, is enhanced by splendid axonometric reconstructions. The book is intended as an "introductory guidebook to the artifacts of Rome and their context" (p. xvi), and, as such, it meets with considerable success. For students seeking an introduction to all manner of topics, it will be a veritable mine of information. But historians will generally take a less idealized view than Stambaugh of the artifacts' social and historical context.

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WILLEM JONGMAN. *The Economy and Society of Pompeii*. (Dutch Monographs on Ancient History and Archaeology, number 4.) Amsterdam: J. C. Gieben. 1988. Pp. 415.

With a wide-ranging essay that surveys thought on the economic structure of Roman antiquity from the late nineteenth century to the present (and with special emphasis on the work of his mentor Moses Finley), Willem Jongman introduces his long-needed reassessment of the evidence from ancient Pompeii. Was it a variety of the Weberian "consumer city" or of the Weberian "producer city," and how does that answer fit with what we know of the social relationships of its

inhabitants? For his answer, Jongman turns to a series of "artifact" and "simulation studies."

Postulating a territory of two hundred square kilometers and a population of thirty-six thousand of which eight to twelve thousand were urban dwellers (p. 112), Jongman addresses the problem of provisioning. Only cereal crops could meet demand, and they were raised by means that were labor-intensive. Working animals that had to be fed fodder made no sense to the Roman farmer saddled with a family that could work and be provisioned from its own production. With a fixed acreage of arable land, the farmer found himself trapped in a situation where production was high but individual productivity low.

The ancient sources refer to the grape as a hill crop that at Pompeii produced a mediocre wine, and earlier scholarly emphasis on Pompeian viticulture was mistaken analysis of the villas found in the region. In fact, several so-called villas are little more than masonry remains, and those that actually produced wine were concentrated in the hills north of town where the terrain was suitable or along the river Sarnus where eased transportation costs—a significant factor in Jongman's application of location theory (pp. 137–46)—made production feasible. Local wine production is unlikely to have exceeded local demand (p. 133).

Similarly, the theory of large textile production is mistaken. Again the evidence has been inflated. Shops and workers have been overenthusiastically identified, yet even so the percentage of workers postulated (less than 10 percent of the population) is far below that of textile-producing cities of other comparable periods (60 percent of the work force, for example, in medieval Ghent). There is no evidence that fullers became entrepreneurs (in comparable situations they did not), and none that they entered, let alone became an important factor in, the *ordo decurionum*. The Building of Eumachia has never been proven to be a guildhall or cloth market (see L. Richardson, "Concordia and Concordia Augusta: Rome and Pompeii," *Parola del Passato* 33 [1978]: 260–72, unknown to Jongman).

In fact, the wide diversity of trades at Pompeii is itself an argument that it was a variety of "consumer city" that could feed a substantial urban mass and satisfy an urban elite accustomed to its luxuries. But the emphasis on production rather than productivity that allowed it to reach this level also cut it off from further development (p. 203).

With this, the evidence of social relationships jells. Pompeii was a city in which rank was finely perceived, as demonstrated by J. Andreau's studies of the wax tablets of Caecilius Iucundus, further mined by Jongman for evidence on social status. There was a great chasm between the rich and powerful and the poor and powerless. Wealth, however, was requisite for, not determinant of, high status; office holding was significant (p. 269); and libertine status excluded one from power (p. 273). Election to the *ordo* was controlled by patronage, and any manufacturer or merchant quickly had to adopt upper-class attitudes to be accepted

(p. 281). The elite owned the land and retained control (pp. 143–46).

Such, in broad strokes, are the conclusions of this stimulating research, generally convincing and arrived at via a number of approaches. Qualms and doubts on lesser matters are inherent in the material and will haunt every reader. Can ownership of a wine-producing villa indicate an upper stratum of the elite (p. 146)? Does the floor space of a house correlate with the wealth of its owner (p. 257)? Can *vici* be understood as voting units and *vicini* their members (pp. 302–09)? But such questions will only stimulate further work, for which the admirable bibliography (pp. 369–98) will prove invaluable.

It is perhaps churlish to criticize the prose of a Dutch monograph published in English, but *caveat lector*: this is a daunting work to read. The book, however, is one every specialist on Pompeii and every Roman economic historian will read and contend with for some time to come.

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MARIO BRETONE. *Storia del diritto romano*. Bari: Laterza. 1987. Pp. viii, 507.

The title of this book is slightly misleading; Mario Bretone offers us not a comprehensive history of Roman law but a series of essays intended to introduce the subject and to provoke thought. The range of these essays is impressive; Bretone discusses the nature of law in the archaic period, the rise of jurisprudence in the late republic, and its fate under the emperors, from Augustus to Justinian. The topics addressed are sometimes the traditional ones of Roman legal history, such as the Twelve Tables, the *responsa* of private jurists, and the role of jurists within the imperial bureaucracy. Other topics are more explicitly adventurous; Bretone is at his most stimulating, I think, when exploring the connections of law with intellectual and political life, with commerce, and with religion.

All of his essays, however, are ambitious. The writing of legal history can easily suggest that developments in the law were prompted simply by the internal logic of a system; as in the intellectual history of a few generations ago, legal concepts tend simply to roll down through the ages. Bretone is determined to resist this trap. His purpose is to consider developments in the legal system as part of, and as evidence for, more fundamental changes in the structure and the mentality of Roman society.

It is perhaps not surprising, given these very ambitious aims, that Bretone is more successful at raising questions than at answering them. The most disappointing thing about these essays is their tendency to make assertions rather than arguments, as if the connections between the legal evidence and the evidence for society as a whole were self-evident. Throughout

the book, in fact, there often seem to be two distinct bodies of information, social and legal, which are juxtaposed rather than interwoven. Thus, a chapter on law in the Later Empire ("Il diritto codificato tardo-antico") offers basic information about imperial government and its bureaucracy and a good survey of the legal sources surviving from the period but does not really investigate the connection between these two bodies of information. The reader is left to infer, I suppose, that bureaucratic government produces a bureaucratic legal literature, which is fair enough but does not take us much further than did, say, the more traditional Fritz Schulz (*History of Roman Legal Science* [1946]). It may be, of course, that with such intractable material any high expectations are almost bound to be disappointed.

These essays, we are told (pp. vi–vii), originated in lectures to law students, and this may account for what at times can seem like mere sententiousness. Readers of this journal may find themselves wishing that Bretone had been less conscientious in providing historical background. A chapter on the response of lawyers to the requirements of commerce ("Oltre il formalismo") begins with salutary but ponderous references to Fernand Braudel and the Mediterranean, Socrates and his frog pond, and even the Minoan thalassocracy. This turns out to be the background for a discussion of the rise of the praetor's edict, the *ius honorarium*, which is an interesting juxtaposition, but it is easy to become impatient.

Perhaps the most obvious readers for this book, at least in this country, will be scholars and students in search of research topics. Bretone's energetic approach to his subject cannot fail to stimulate interest in the potential of the Roman legal material for the writing of social, economic, and intellectual history. Simply pursuing connections that he suggests would be interesting and might well yield results. He provides, as well, a very good appendix on bibliography.

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WILFRIED NIPPEL. *Aufbruch und "Polizei" in der römischen Republik*. Stuttgart: Klett-Cotta. 1988. Pp. 334.

Wilfried Nippel's book joins the numerous recent studies of political violence and its suppression, which for understandable reasons have become fashionable in many branches of history. Although the book is meant to be accessible to nonspecialists (there is a glossary of technical terms at the end), the reader will quickly become aware that it forms part of a vigorous debate among scholars interested in the Roman constitution and public law. On those topics the book has the merit of being a good summary of much current thinking, in addition to pursuing important themes of its own.

In the book's first section, dealing with the problem of maintaining security at Rome, there are two main themes: first, the lack not only of a police force but also

of a concept of policing in the modern sense; and second, the crisis measures used against political turbulence. Under the first heading, Nippel discusses the limitations of the concept of *coercitio* and of the forces and procedures available to magistrates; under the second, he discusses matters such as laws against violence, the annulment of laws passed by force, the raising of irregular forces in an emergency, and the so-called ultimate decree of the senate. In the second section of his study, Nippel shows how such crisis measures were used successfully against the Gracchi, Saturnius, and the Catilinarian conspirators. In the third section, which centers on the violence of Publius Clodius between 50 and 60 B.C., the author shows how Clodius's organization of the urban *plebs* broke traditional methods of aristocratic control, leading to the use of troops as police and unbalancing aristocratic politics. Then, after a look at the importance of the urban *plebs* in the aftermath of Caesar's murder, Nippel sketches the situation under the monarchy that followed the civil wars. Here he stresses that the existence of an authority to which a police force could be subordinated did not in fact create peace and security for ordinary citizens.

Nippel is a student of Christian Meier, and that is reflected both in the book's structure, which resembles that of Meier's *Res Publica Amissa* (1966) with a discussion of norms and exceptions that is followed by a detailed examination of particular crises, and in some of its presuppositions. The Roman Republic is seen as an aristocratic regime that ultimately could not cope but that for a long time managed to respond flexibly to the problems posed by its own members and the *plebs* on whose back it stood. The importance of plebeian self-consciousness before the time of Clodius is arguably underplayed and the stability of the middle Republic overplayed. A fundamental reconsideration of the concept of *imperium* inside the city would have been helpful, and, although Nippel's strictures on excessively modern concepts of police are well taken, his own notion of magistrates exercising authority by tradition, ritual, and consent needs further examination. That notion obviously holds true some of the time and up to a point, but up to what point? Although the Romans obeyed authority as soldiers, it does not follow that they were disciplined as civilians. Here the norms of behavior in private life are surely relevant.

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AARON KIRSCHENBAUM. *Sons, Slaves, and Freedmen in Roman Commerce*. Preface by ALAN WATSON. Washington, D.C.: Catholic University of America Press or Magnes, Jerusalem. 1987. Pp. xii, 229.

This book, which began as a doctoral dissertation completed in 1969 under the aegis of Arthur Schiller at Columbia University, originally bore the more prosaic

and indicative title "Non-Contractual Agency in Roman Classical Law." In it Aaron Kirschenbaum begins with the old problem of why a legal system as complex as the Roman one failed to develop a sophisticated legal theory of agency. His answer is a traditional one: that the close ties of dependency within the Roman *familia* and the powers that a father was able to exercise over sons, slaves, and other familial dependents were sufficient to guarantee those dependents' actions as agents without the need of any elaborate legal apparatus to tie them by formal contractual constraints. That idea was already available in handbooks of the Roman law and even in beginners' textbooks, such as Barry Nicholas's *An Introduction to Roman Law* (1962).

Kirschenbaum adequately reviews the significance of the *peculium* or "allowance" set up for sons and slaves by fathers and masters and in what ways that purse could be used by those human extensions of the master's personality to run small businesses, craft shops, and shipping agencies. He also reviews the legal actions that were permitted to those who had claims against agents who used their *peculia*, outlining the differing liabilities of sons and slaves. Kirschenbaum indicates that, in certain circumstances (where the principal profited, even indirectly, from his agent's business dealings), the legal action allowed (*in rem verso*) admitted a relationship close to that of modern agency, the plaintiff being able not just to sue to the limit of the *peculium* but even to proceed against the capital of the principal himself. Both in regard to the praetorian development of the law and to the extension of agency to the large social group of freedmen, however, the author does not offer sufficient analysis of Roman agency as something distinctive and independent of its modern counterpart.

Much of the modern scholarship that would be directly relevant to that problem is absent from Kirschenbaum's analysis. The most recent item in the bibliography dates to 1981, the next most recent to 1977. In fact, most of the author's recent scholarship is limited to the 1960s. Since the original dissertation, completed in 1969, apparently has not been much revised, the book misses out on the flowering of scholarship in the last decade on precisely the subject in question. Fundamental articles (for example, Peter Garnsey, "Independent Freedmen and the Economy of Roman Italy under the Principate," *Klio*, 63 [1981]: 359–71) on problems concerning the social status and legal position of freedmen are absent, as are books that discuss the problem of the Roman concepts of friendship and patronage that Kirschenbaum considers to be such an essential social backdrop to the "quasi-agency" we see in Roman law and social practice (for example, Richard Saller, *Personal Patronage in the Early Empire* [1982]). The exposition, at least its social history side, has a rather archaic, if not confused, tinge to it, with references to Marcel Mauss's gift-giving primitives set side by side with Tenney Frank's rampant capitalists in explanations of Roman behavior. Perhaps a greater lacuna, however, is the object of comparison: the modern legal conception of agency. There is hardly a

word about the conditions under which the modern legal idea emerged, how it came to be commonly applied, and, more important, whether its modern function was necessarily provoked by the same scale of business concerns as Kirschenbaum regards as common for the Roman empire. In fact, as late as the eighteenth and nineteenth centuries, a whole range of small business concerns in Western Europe were run by precisely the same type of familial interests that the author regards as the reason for the lack of "true" agency in the Roman empire. What is needed, therefore, is more of the modern half of this story (for example, Peter Stein, "Mutual Agency of Partners in the Civil Law," *Tulane Law Review*, 33 [1959]: 595-606) in order that the Roman case might be understood more properly in its legal and historical context. For example, Kirschenbaum fails to make clear just how hypermodern the development of the modern conception and law of agency is. In the common law tradition, the modern law of agency did not begin to be formulated until the early nineteenth century, and its full impact was not reflected in major legislation until the 1880s and 1890s in conditions demanded by an economic system forged under the impact of the second industrial revolution. Before that time whatever law of agency existed was happy to refer to norms established by the Roman law.

This book is a useful and readable summation of some known aspects of the Roman law, together with some rather dated explanations. Nowadays one has come to expect the high caliber of research provided by, say, Bruce Frier in works that expertly bridge the world of social practice and legal theory in a way that illuminates both. Kirschenbaum's omission of almost all recent research on the noncontractual aspects of Roman agency greatly detracts from his book's potential value to students interested in a new and exciting topic of Roman history: the social history of the Roman law.

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PAUL ZANKER. *Augustus und die Macht der Bilder*. Munich: C. H. Beck. 1987. Pp. 369.

Although the age of Augustus is the best-documented period of ancient history, its central figure, who had the longest hold on power in Rome's long history, has been until recently characterized as an enigma: elusive, baffling, even inscrutable. Even if he was a unique individual in a unique period, we now have sophisticated techniques (studies on the nature of leadership, on the typology of political personalities, and on power wielders) for assessing Augustus's motives and methods. It was Theodor Mommsen's verdict that "Augustus adroitly played the role of the great man without himself being great" (see Meyer Reinhold, "Augustus' Conception of Himself," *Thought* 55 [1980]: 50). What Mommsen and others have overlooked is that the

"real" Augustus was obscured by his genius as myth maker, by his consummate skill in the creation and diffusion of symbols, signs, and images as visual communication to unify the "new order" that he molded from the ashes of the moribund Roman Republic.

In this absorbing landmark work (now available in an English translation), Paul Zanker describes and analyzes the origins, nature, evolution, and power of those myths as spread over the visual imagery of the Augustan Age. He displays masterful control of the literary sources, architectural monuments, sculpture, coins, and minor artifacts. The "language of images" (p. 5) from the last decades of the Republic through the reign of the first princeps is the theme of this comprehensive work.

As prelude, Zanker examines the diversified, eclectic, contending, individualized imagery generated by the elite of the end of the Republic, an elite that was characterized by private lifestyle, luxury spending, and private building. The transition to the Augustan value system occurred in the competing imagery of Antony and Octavian in their quest for sole power, a conflict, as it were, between a Dionysiac and Apollonian creed.

The grand shift to a unified cultural and imperial imagery, prefigured in Octavian's youth, burst forth soon after the Battle of Actium and the suicides of Antony and Cleopatra. The success of that imagery in dominating the iconography and visual messages of the ensuing half century was overpowering and ubiquitous, despite the profound contradictions of the age: for example, the *res publica restituta* and the overriding powers of the princeps, who was the prime decision maker of the empire; the annual election of public officials and the reality of an imperial dynasty; the revival of the decaying state religion and the growing emperor worship; the call to moderation in luxury spending and Augustus's own taste for the colossal, especially in his mausoleum, the largest structure of his reign, built in a conspicuous place in Rome when he was only about thirty-five years of age; his frequent declaration of *consensus universorum* in the face of incipient conspiracies against him and uprisings in the empire.

The unity of the images of the Augustan Age is here insightfully analyzed by Zanker: its general "upbeat," "feel good" pattern; the religious restoration together with the marriage of the altar and the throne; the Augustan Age as a "golden age"; moral restoration with emphasis on Roman national traditions, the *mos maiorum*; glorification of Augustus as supreme arbiter and of the imperial family; peace and prosperity; fertility and stability; Augustus as defender of *libertas*, paternalistic leader, *pater patriae*, and even savior of the world.

The Augustan value system is exemplified in Augustus's vast building program for the imperial capital, with its interest in public buildings; in the Altar of Peace, the supreme embodiment of Augustan imagery; in his official portraits combining the aesthetic canons of Greek art and Roman content, idealized to suggest

timeless patterns. Augustus's unitary system of visual communication, with its new language of images, was both prescriptive and reflective of the mentality of the age. The messages were those of the new order; the media were as diversified as the ancient modes of communication allowed. The images embraced not only the present but also the ancestral past, and they set up patterns for the future. As Zanker points out, the price of Augustus's overpowering official imagery was the stifling of imagination and creativity for the next two centuries in many areas of Roman cultural life. We look forward to Zanker's planned work on the "language of images" during the first and second centuries.

Zanker's study of Augustan imagery is a masterpiece of historical writing and combines cultural history, art history, and archaeology. One hesitates to cavil at a minor error, but the absence of an index of names, places, works of art, and concepts is noteworthy.

Edward Gibbon called Augustus "that subtle tyrant," and Ronald Syme called him "that small town bourgeois" who gave the Romans "the peace of despotism." Perhaps one might apply to Augustus what François Chateaubriand said of Napoleon: "It was not enough to lie to their ears, he had to lie to their eyes as well." Augustus might have countered with *exegi monumentum*.

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H. R. ELLIS DAVIDSON. *Myths and Symbols in Pagan Europe: Early Scandinavian and Celtic Religions*. Syracuse, N.Y.: Syracuse University Press. 1988. Pp. xii, 268. Cloth \$28.00, paper \$15.00.

H. R. Ellis Davidson's work on German myths is well known to scholars and to a wider general audience, particularly through *Gods and Myths of Northern Europe* (1964), a standard in the field. Her new volume should be equally celebrated, for it is a wonderful and wise book, the product of a deeply learned and down-to-earth scholar.

The book's thesis is that one cannot know the past without understanding the ideas that governed the mental worlds of the people who lived at any given time. The people in question are ancestral Europeans; therefore, to examine their ideas is to explain something about those that underpin our own mind-sets. Myths and folklore are the tantalizing remains of that past, for they recall bits of history, religion, and ritual encased in traditional forms. By combining studies of myths and folklore with linguistics and archaeology into a coherent approach, the author gives the reader a vivid and highly suggestive vision of societies lying just beyond the full light of recorded history.

Davidson examines aspects of two sets of mythic systems, Celtic and Germanic/Scandinavian, and demonstrates the powerful connections between the two. Thus, she pushes back whole sets of ideas found among recently Christianized northern folk to the early Iron Age when Celtic and Germanic peoples may have been

one. The author wisely restricts the time span and geographic scope of the study, for that restriction eliminates the excesses—some mystical, some ideological—that have plagued some works on European prehistory. Concerned mainly with religion rather than narrative history, the book divides its subject matter into broad categories of ritual and belief. Holy places, feasting and sacrifice, land-spirits and ancestors, foreknowledge and destiny, and the other world are among the main subjects. Within each, Davidson shows how symbols, whether revealed in icons such as stone pillars and art or by word and gesture, tell us much about the mental picture the ancients might have had.

A graphic picture it is. Here were people living a precarious existence, always at the mercy of unseen powers that might bring good or chaos. Observation of the regular succession of the seasons, elaborate rituals, spells, and incantations to supernatural protectors were deeply embedded in these cultures. On that basis, Davidson makes logical what has often seemed inexplicable in ancient practices such as human sacrifice. Her interpretations are always careful and sensible. The Tuatha De Danann in Irish tradition, for example, are not a historical folk but more likely ancestral spirits of the land.

Potent images remain when the book is done: battle goddesses; the coinciding powers of Odin and Lug and their signs, such as the eagle brooches of the Migration Period; the cult of the head with its many gruesome icons; Scandinavian or Irish chiefs addressing their people from atop burial mounds of ancestors whose spirits were very much alive. Such is the nature of this splendid book.

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MEDIEVAL

JACQUES LE GOFF. *The Medieval Imagination*. Translated by ARTHUR GOLDHAMMER. Chicago: University of Chicago Press. 1988. Pp. vii, 293. \$27.50.

In his introduction to these sixteen essays (published together in French in 1985, although some of the essays first appeared as early as 1974), Jacques Le Goff passionately asserts that his historical territory takes us to the core of human and historical experience: "To study the imagination of a society is to go to the heart of its consciousness and historical evolution. It is to go to the origins and the profound nature of man, created in the 'image of God'" (p. 6). Le Goff makes it clear that his approach to the culture of the Middle Ages is decidedly not Marxian: works of the imagination are not simply ideological reflections or representations of social and economic reality; the imagination sometimes shapes social, political, and economic reality.

Leaving out the visual arts (because he does not consider himself sufficiently trained in that field), Le Goff explores the medieval imagination through a variety of literary and theological sources. Those who

are familiar with his work will recognize both his sources and his themes: exempla literature, dream literature, courtly literature, perceptions of time and space, the city imagined, the birth of purgatory, linguistic and behavioral codes.

The essays reveal at least two key aspects of Le Goff's method. First, as he himself claims, he gives priority to the history of words: "The history of words is history itself. When terms appear or disappear or change their meaning, the movement of history stands revealed" (p. 12). Le Goff employed this philological method in the *Birth of Purgatory* (English translation, 1984). Here he points to such a method in his essay titled "The Marvelous in the West," where he claims that, in the twelfth century, theologians began to make increasingly clear distinctions between "marvels" (*mirabilia*) and "miracles" (*miracula*). Miracles, Le Goff asserts, had God as their author and fit into his plan. Marvels had closer ties with folkloric culture and were increasingly dismissed as superstition.

A second element of Le Goff's method entails an effort to combine a structuralist's eye for codes ("vestimentary," alimentary, linguistic) and oppositions (nature and culture; country and city; left and right; father and son; inside and outside) with a historian's eye for change and context. He asserts, for instance, that, while ancient society emphasized the opposition between city and country, medieval society emphasized the opposition between nature (that which is wild) and culture (anything that is built, cultivated, or inhabited). In Chrétien de Troyes's *Yvain*, moreover, the mediating realm of the clearing and hermit (half-way between nature and culture) was related to the actual practice of land clearance in the eleventh and twelfth centuries.

To my mind, the most interesting and successful essays in this volume are "Christianity and Dreams (Second to Seventh Century)" and "Warriors and Conquering Bourgeois: The Image of the City in Twelfth-Century Literature." In the essay on dreams, Le Goff displays, once again, the careful methodology that he employed in the *Birth of Purgatory*. Through an examination of a large group of pagan and early Christian sources, he convincingly suggests that Christian theologians both incorporated and transformed the pagan tradition on the nature of dreams. The most striking transformation involved predictions about the future: although Christians, like their pagan predecessors, continued to believe that divine messages could be sent through dreams, they ceased to believe that dreams could foretell the future, because the future belonged "to God alone" (p. 213).

In "Warriors and the Conquering Bourgeois," Le Goff draws on a number of works of twelfth-century courtly literature from which he elicits three knightly attitudes toward the city: lust (the city as woman or prey, an object of desire to be seized); idealization (the city as an ideal space in which warriors and burghers lived in harmony); and hostility (the city as a center of economic, social, political, and cultural developments that were hostile to the knightly class).

For an audience of Anglo-American historians, Le Goff's essays, especially the shorter ones and those with only a minimum of footnotes, can often be as frustrating as they are stimulating. In part, the frustrations result from the differences between Anglo-American and French academic "codes" and cultures, as well as from our own distance from the French academic social experience.

First, a number of Le Goff's essays suffer from decontextualization: they read like oral presentations (even conversations) that have been captured by, but not transformed into, the written mode of communication. Frozen into print and translated into English, lively conversations on exciting topics—topics that are apparently still under investigation at the Ecole des Hautes Etudes—lose much of their original meaning and significance. A reader who is not in direct contact with Le Goff's oral culture can become baffled by some of his assertions, distinctions, and categorizations. I am willing to be convinced, for instance, that sometime around the twelfth century theologians began to distinguish "miracles" from "marvels." I am not given enough evidence, however, to judge whether or not the assertion holds. I am further confused, moreover, by the fact that Le Goff's own discussion does not sustain the distinction between miracle and marvel: as an example of a marvel, he cites a story (one that seems to fit his definition of a miracle) from Caesarius of Heisterbach's thirteenth-century work, the *Dialogus miraculorum*. Where, then, in either name or concept, is the clear distinction between the two?

Anglo-American readers might also have difficulty with some of Le Goff's prose. At times (but certainly not always) his essays evolve into lists and catalogues with no clear arguments or conclusions. I assume that at these points the mode of exposition is that of a structuralist. Similarly, it seems to be the structuralist who wants to declare, "I préfère to study the imagination by means of science, not mystification" (p. 5). To those of us who stand outside of French academic culture, this claim might seem a bit puzzling. At his best, Le Goff pushes us beyond pure "science": he stimulates the imagination, enabling us to perceive medieval culture in ways that we had not perceived it before. And, in his weaker moments, he falls short of demonstrating that he is a practitioner of "science," which we would associate with clear evidence and factual precision rather than with structuralist codes.

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WARREN TREADGOLD. *The Byzantine Revival, 780–842*. Stanford: Stanford University Press. 1988. Pp. xv, 504. \$49.50.

Although invited to do so, the nonspecialist will find this a difficult book to read. Warren Treadgold, who claims to be as comprehensive as possible, has let

practically nothing recorded to have happened between 780 and 842 escape his notice. The result is a highly detailed narrative of six decades of Byzantine history. The sources for this period are not abundant or wholly reliable, but Treadgold has extracted whatever information he can from them, and, for the most part, his own surmises are properly qualified.

The book is well organized. An introductory chapter describes the empire in 780, its territory, administration, economy, and the like. The bulk of the book consists of four narrative chapters. The first is devoted to the twenty-two years that the empire was ruled by a woman, Irene, to whom Treadgold gives higher marks than have previous scholars. He also has a better opinion of her successor, Nicephorus (802–813), and stresses his administrative and economic reforms. A devastating civil war, assassinations, religious strife, and military defeat predominated in the years from 813 to 829. The account concludes with the cultured Theophilus, whose early death in 842 brought his ambitious plans to an inconclusive end. Although one might dispute an interpretation here and there, these chapters make up a complete history of the period, with less attention being paid to religious and cultural developments. The final chapter presents the state of the empire in 842.

Not all Byzantine specialists would agree with the opinions stated in the introductory chapter. Most scholars postulate a break in continuity between the late Roman empire and its Byzantine successor, and most center this break in the seventh century. That this break necessarily implies a decline is open to question. Further, if one is to speak of a decline, can one prove that it set in around the end of Justinian's reign and was caused by the plague that began in 542? More persuasive argumentation is required. Whether or not one employs the term "revival," as in the book's title, it is clear that new developments gave the Byzantine empire its distinctive characteristics. Many scholars find these developments much earlier in the eighth century. Treadgold's reasons for settling on 780 as the beginning of the revival are by no means clear or convincing.

Heavily documented as this book is, one would like to see more justification for certain statements. How do we know that the Byzantines were demoralized by their misfortunes and by theological disputes about them (p. 3)? How do we know that the empire's population was increasing (p. 4)? The note on page 257 does not corroborate the statement that iconophile sentiment remained widespread. The reader may want to know why the drawings of the emperors in a fifteenth-century manuscript "seem to have some portraitive value" (p. 4). Sometimes the note at the end of a paragraph documents all of the items mentioned; sometimes it does not. One is frequently left wondering on what authority a statement has been made. In a few instances the sources may have been misunderstood. Hagia Eirene served as a cathedral only temporarily during construction at Hagia Sophia (p. 22). The Stephen who proposed reading fifteen more books on

icons at Nicea II was not one of Tarasius's notaries but a monk (p. 86). In some instances the references given in the notes are off by a page or two.

One very annoying feature of the book is its latinizing of Greek names and terms in the classicist manner, an affectation long discarded by most Byzantine scholars. Why Panormus (pp. 250, 274), for example, when Palermo, or even Panormos, would be much better? Why the monstrous Cephaloedum for Cefalù (p. 306)? It makes no sense to force Latin endings onto Greek (or originally Germanic) words: for example, "strategus," "bandum," "drungus"; the correct forms are "strategos," "bandon," "droungos" or, if one prefers a translation, "general," "band," "throng."

The notes are printed at the end of the book, which concludes with a glossary of terms, a bibliography, and an index. There are several diagrams and maps, with everything fitting neatly, almost too neatly, into place. The illustrations, including that on the jacket, have been carefully selected and nicely reproduced. The press should be complimented on producing an attractive volume. While bearing in mind the above reservations, it must be admitted that this is a work of painstaking and substantive scholarship and should long remain the authoritative work on the period.

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RICHARD HODGES. *Primitive and Peasant Markets*. (New Perspectives on the Past.) New York: Basil Blackwell. 1988. Pp. xiv, 175. Cloth \$34.95, paper \$12.95.

Those who have read Richard Hodges's books on the economics of Europe in the Dark Ages will not be expecting easy reading from his latest work. This book is tightly argued and densely written, and there are passages with which one has to struggle in order to extract any meaning. Yet it is worth the effort. Hodges is highly theoretical in his approach, and his intent, broadly speaking, is to build models of how trade began and developed.

"The literature on the formation of markets," writes Hodges, "is remarkably thin" (p. 78). There are no documentary sources that reveal how markets were established and trade organized either in prehistoric society or in the Dark Ages. Reliance has therefore to be placed, first, on archaeological evidence and, second, on current conditions in Third World countries that can, with reservations, be treated as surrogates.

The book opens with a discussion of the typology of markets in which models, most of them elaborated by anthropologists and sociologists, are examined and compared. Many readers will find this theorizing tedious and far from meaningful. Authorities are pitted against one another, and their contests are left unresolved. In this discussion one name, Carol A. Smith, is cited or quoted some fifty times, and Smith must be judged the winner on points. The author then approaches the most "primitive" and, presumably, the

oldest markets. He discusses "unequal" trade, the development of "gateway" communities established on the margin of a less developed territory, and the kind of trade that consists, in effect, of reciprocal gifts. All are seen as stages in the development of a market system, and all can be illustrated by Europe in the Dark Ages. A market was a far more complex institution than that portrayed, for example, by Henri Pirenne after the eclipse of the Roman empire in the West. There was no universal propensity to trade awaiting the creation of a market by an enlightened individual. There were, at first, no specialists waiting to exchange their products. Both specialization and trade had to be coaxed into existence and chiefly by the elite for purposes of their own conspicuous consumption and conspicuous waste. No one who studies prehistorical society or society in the Dark Ages can afford to ignore this structural analysis of market systems.

Markets have come to presuppose a currency, and one glibly assumes that coins were minted merely as a medium of exchange and a measure of value. Not at all. Coins served many purposes. They were used for propaganda, for example, by Roman emperors, whose features, stamped on thin metal disks, thus passed from hand to hand. They were worn on the person for reasons of pride or personal adornment. I well remember a grandfather who wore conspicuously on his watch chain a spade guinea of George III.

Early coins represented too large a sum to be very useful in primitive markets in which barter long survived the introduction of cash. Only when the concept of cash had become generalized were fractional coins minted, and then, with inflation, the need for them disappeared and "grossos" and "groats" came into circulation. I found the discussion of money and value in primitive markets one of the most intriguing and valuable parts of the book. It comes toward the end. I can only advise a would-be reader to struggle valiantly through the pages of rather sterile theorization, for there is a real world of peasants, their money, and their markets looming at the end.

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GEORGE LAWLESS. *Augustine of Hippo and His Monastic Rule*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xix, 185. \$48.00.

Putting the right title on a book is not as simple as it seems. This one, for instance, would have been more appropriately dubbed "Studies in Augustine and His Rule." Not a connected account, the book consists of three separate and virtually independent sections, only the first of which—a sixty-two-page essay titled "Augustine's Mind and Milieu, 386–396"—has anything of the scope normally associated with a book-length study (part 2 consists of text and translation, with brief introduction, of four of the nine documents that constitute Augustine's Rule, and part 3, dealing with

disputed questions of authorship and date, is virtually indistinguishable in breadth of subject matter from the two appendixes on a later version of the Rule and authorship of the *Ordo* that conclude the work).

This is not a book for beginners. Even in part 1, George Lawless tends to treat issues in a way that presumes his readers already are familiar with the broader framework of Augustine's life and the problems that specialists have raised. Commenting on, referring to, disagreeing with, but rarely summarizing the work of other scholars, Lawless leaves the general reader with the uneasy sense of having walked in on a private conversation.

The aim of part 1 is to show, first, that Augustine's ascetic interests were the product of a shared pagan-Christian environment and, second, that the communal life Augustine and his companions adopted at Thagaste upon his return to North Africa in 388 may be legitimately characterized as monastic. The first point is made ably enough, although the reader would never know from the discussion why it needs to be made at all. But key parts of the argument for the second point are either missing or muddled. In discussing the extent to which Thagaste might be characterized as a monastery, for instance, Lawless makes a distinction between "ascetic" and "monk" on page 55, points out on page 57 that the ascetic's practice of chastity and poverty must be supplemented by "some emphasis upon a life shared with others in the service of the Church," and concludes on page 58 that Augustine should not be held to the standard established by St. Benedict nearly a century later. Each of these points by itself is legitimate and useful. But they seem to work against each other: if the Benedictine comparison is not relevant, is "service to the church" (obedience?) so vital a criterion? Moreover, it was not clear to me that the preceding description of life at Thagaste demonstrated that this criterion had been met.

Lawless is at his best in dealing with the more closely focused questions of manuscripts, dates, and authorship in part 3. Clearly at home with the evidence, Lawless ably lays out the issues and draws judicious conclusions. Yet even here he is as inclined to catalogue the decisions of other scholars as he is to weigh the evidence on which their decisions were based.

In line with R. G. Collingwood's analogy of history as a "game" in which the winner is not the player who is "right" but the one who has made the most persuasive case, the best that can be said on the basis of the evidence presented here is that too often Lawless's case is "not proven." Consequently, all but the most specialized readers probably will limit their use of this volume to the translations in part 2 and will turn to the author's excellent bibliography for fuller discussions of the issues he raises.

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GERD TELLENBACH. *Die westliche Kirche vom 10. bis zum frühen 12. Jahrhundert*. (Die Kirche in ihrer Geschichte, volume 2, part F1.) Göttingen: Vandenhoeck und Ruprecht. 1988. Pp. xii, 272. DM 48.

Gerd Tellenbach has been counted among the chief representatives of the German historiographical tradition, together with Wilhelm von Giesebrecht and Karl Hampe. Published in Tellenbach's eighty-fifth year, the present book is a contribution to a series entitled "The Church in Its History." It is one (and now the longest) of seven projected fascicles in a volume on ecclesiastical history in the Latin West from the sixth to the sixteenth centuries.

Tellenbach displays, with a firm, sure touch, enormous erudition. He has brought to bear an extraordinary mastery both of literary evidence surviving from the period under review and of scholarly literature. The book stands on a massive *apparatus criticus* built with meticulous attention to detail over a period of fifty years. Regrettably, the publisher omitted an index; for some elementary and other purposes, the virtues of this extremely detailed book are thus rendered inaccessible.

The expanse and solidity of the *apparatus* is the result in part of the unswerving thoroughness with which, over the decades, the author has investigated the same two propositions, which he set forth during the 1930s. These are the informing ideas of the book. The earlier of the two (argued in Tellenbach's celebrated *Habilitationschrift*, *Libertas, Kirche und Weltordnung im Zeitalter des Investiturstreites* [1936]) is that the period between the mid-tenth and the early eleventh centuries constitutes a distinct era in the history of the Latin church. According to this proposition, the Investiture Controversy was the crucial moment in the transformation of the Western church, a transformation that was, in fact, "the cumulative actualization of the papal idea" that Rome held a "universal episcopate" (pp. 224, 241). Pope Gregory VII was the chief agent of change. Thus, Tellenbach established an equation between church and papacy. The second proposition was the core of his book *Die Entstehung des deutschen Reiches* (1940). Written in the Third Reich, this study evidently did not blemish Tellenbach's tenure at Giessen or impede the rich flowering of his career in professorships at Münster (1942–44) and Freiburg (1944–63). Free as it is of Nazi ideology, the book yet imbibed enough of the dominant ethos to set forth the double proposition that *Volk* and *Reich* were inseparable entities, understandable only in their mutual relations, and that the political order of the German *Reich* was the keystone of all European history in the formative period between the sixth and the tenth centuries.

It is not surprising to find the huge collection of data in *Die westliche Kirche* organized by ideas, for, throughout his search for the concrete, Tellenbach has examined constitutional and social facts as media for *Geistesgeschichte*. Nor is the impermeability to change of his two key propositions surprising in view of the fact that

he kept the methods and goals that he pursued on the frontiers of prosopography independent of those advanced at the *Annales*. This single-mindedness, however, detracted from Tellenbach's very considerable achievement in some details when it enabled him to maintain interpretations that, over the years, have become doubtful (for example, Gregory VII's authorship of the *Dictatus Papae* and the *Dictatus* as "an epoch-making expression" of the thought that drove the papacy to establish itself as a universal episcopate [pp. 238, 241]) and to buttress others insecurely (for example, confirming his concept of Gregory VII's motives by referring to a distant chronicler's account of a death-bed statement ascribed to the pope).

Perhaps, on another level, the two informing propositions were entirely cogent, even in the 1930s, only from within the German intellectual world. Now a rather more complex and inclusive interpretive schema is needed. On laying down this book, one wonders, for example, whether there were any women in the Western church. And yet Tellenbach expressed an essential truth when he concluded an early sketch for this book (cited on page 1) by saying that historians could not eliminate themselves from their interpretive methods. Instead, he wrote, they had to put themselves frankly into the question, together with the object, however distant and surprising, with which they wished to gain rapport.

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MARY STROLL. *The Jewish Pope: Ideology and Politics in the Papal Schism of 1130*. (Brill's Studies in Intellectual History, number 8.) New York: E. J. Brill. 1987. Pp. xviii, 205. \$54.00.

Mary Stroll's study of the papal schism of 1130 takes a revisionist view. Instead of seeing the two claimants to the papacy as champions of different ideological factions among the cardinals, she regards this disputed election as a political struggle resulting mostly from the efforts of the papal chancellor, Cardinal Haimeric, to maintain his preeminent influence in the determination of papal policy.

The leading proponents of the "ideological school"—Hans-Walter Klewitz, Franz-Josef Schmale, and Stanley Chodorow—have proposed that steadily sharpening differences of origin, affinity, and viewpoint emerged among the cardinals during the two decades immediately preceding and following the Concordat of Worms (1122). According to these historians, two parties indeed formed: a group of intransigent "high Gregorian" anti-imperialists loath to permit any interference by lay rulers (the emperor, above all) in the selection and installation into office of bishops and abbots; and a less senior group willing to compromise with Henry V regarding investitures.

Stroll assaults this construct from all sides, examining

its various premises in some detail. As a minor theme, she also explores how the pope of Haimeric's faction, Innocent II, managed to secure the recognition of the great majority of rulers and churchmen in Western Europe, while the prospects of his rival, Anaclet II (Peter Pierleoni), dwindled to insignificance even before his death in 1138. Most prominent among Anaclet's political weaknesses, Stroll asserts, was "his vulnerability as a scion of a family of wealthy Jewish converts" (p. 181). His enemies could use all of the weapons of medieval anti-Jewish polemic to discredit his personal worthiness for the papacy—hence, the title of the book.

According to Stroll, there is no evidence that Pierleoni was ever alienated from Pope Calixtus II personally or that he dissented from that pope's policies either before or after the Concordat of Worms. Thus, Pierleoni seemed an unlikely leader for any allegedly intransigent faction of cardinals. Nor can the downfall of Abbot Pontius of Cluny during the mid-1120s be seen in terms of ideological disagreement in the church over monastic reform, that is, as a harbinger of the schism of 1130. Furthermore, adherence to differing principles did not dictate either Innocent's or Anaclet's search for political allies after 1130. Anaclet's increasing reliance on Roger of Sicily, whom he crowned king, issued from simple necessity, not a special affinity for an anti-imperial, pro-Norman Gregorianism. As each successive European prince declared for Innocent, Anaclet was compelled to embrace his only important secular ally even more tightly.

Stroll includes a careful analysis of both the specific circumstances of the two papal elections in 1130 and the legal arguments advanced thereafter by partisans of both popes. Neither election, according to Stroll, was wholly canonical. Nonetheless, Innocent's election allegedly exhibited graver divergences from legal norms, for it represented a precipitous attempt by Haimeric to impose the selection of his own candidate, much as the chancellor had done six years earlier when he engineered the irregular election of Honorius II. Stroll's review of the propaganda battle after the elections of 1130 stresses how the Innocentians mostly ignored the legal weaknesses of their pope's election while magnifying those of Anaclet's election. Far more important, however, were the Innocentians' vitriolic attacks on Anaclet's character; this vituperation shifted the terms of debate away from closer scrutiny of Haimeric's motives and actions. In short, Stroll suggests that Innocent's ultimate victory issued from his supporters' more efficient and more ruthless public relations campaign, not from any generally informed decision by churchmen and secular rulers in favor of one of two divergent ecclesiastical ideologies.

Stroll tackles several additional suppositions of the "ideological school." She claims that the Innocentian cardinals of 1130 did not especially exemplify newer currents of spirituality in the church. Instead, most were junior cardinals, many of whom owed their elevation to Honorius II and, Stroll presumes, to Haimeric. The chancellor, despite his close relationship with

Bernard of Clairvaux and Peter the Venerable, does not earn Stroll's respect as a selfless champion of church reform. Haimeric's association with ecclesiastical "entrepreneurs" (p. 120), such as the scheming Archbishop Diego of Compostella, seemingly verged on simony. Innocent II also receives little admiration from the author as a reformer-pope; she notes, instead, his alleged predilection for extravagance and display, combined with an excessive interest in promoting judicial activity at the papal curia.

Anaclet himself provides the focus for the final portion of the book. Before 1130 Cardinal Peter Pierleoni had fulfilled his duties with diligence and honesty, without the slightest hint of opprobrium. But Pierleoni's distant Jewish background provided Innocentian partisans with a weapon whose use, in Stroll's opinion, reflected deep contemporary anti-Jewish prejudices, not merely a convenient tactic in a propaganda campaign. She is unable, however, to combine this conclusion with her observation that no such prejudice was shown to Peter before 1130, when he was already a cardinal. And why did this prejudice not irrevocably hinder the continued prominence of this allegedly discredited family in the thirteenth century?

Despite Stroll's ability to cast doubt on the arguments of the historians of the "ideological school," her book will probably not be the last word. Future discussion will doubtlessly center on her selection and interpretation of rather complex evidence, and in this regard she has provided a highly stimulating and provocative study.

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MAURICE ZUFFEREY. *Die Abtei Saint-Maurice D'Agaune im Hochmittelalter (830–1258)*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 88.) Göttingen: Vandenhoeck und Ruprecht. 1988. Pp. 361. DM 78.

Maurice Zufferey examines the religious, political, and economic development of an important Swiss ecclesiastical center from the point at which it became a house of secular canons in the early ninth century until the death of one of its most successful abbots, Nantelm, in 1258.

The first half of Zufferey's two-part book traces the early history of the house to the end of the Rudolfinger dynasty in Burgundy in 1032. Dedicated to the leader of a group of soldier martyrs killed in the vicinity of Agaune in the third century, Saint-Maurice was established and munificently endowed by the Burgundian king Sigismund in 515. During the Merovingian period, it was renowned for its spiritual life. When the Carolingians came to power, they quickly realized its strategic importance, near the Great Saint Bernard pass, and made sure that it was in the hands of loyal supporters. Like so many religious houses, it suffered in the turmoil of the later ninth and tenth centuries.

Already, however, a new force in Burgundy, the Rudolffinger dynasty, had emerged, and it became strongly identified with the house. Rudolf I was crowned there in 888, and several members of the family were buried at what became the royal religious center of the kingdom.

Although Saint-Maurice diminished in political importance under the counts of Savoy, the regional successors of the Rudolffingers, its spirituality was greatly enhanced by the introduction of the Rule of Saint Augustine, circa 1128. In this second part of the book, Zufferey examines the organization of the house, its relations with churchmen, its economic status, and the importance of the cult of Saint Maurice between the eleventh and mid-thirteenth centuries.

Zufferey's book is solid and provides a valuable agenda for future research on the subject. Yet his work could have been of greater significance in several ways. In the longest chapter, almost one hundred pages on the economic life of the house, nearly three-fourths of the material consists of long lists of transactions. It would have been of much greater value to edit and publish the charters on which this material is based.

A greater problem is the narrowness of the focus. Neither the importance of the cult of Saint Maurice nor the special nature of the house stands out with sufficient clarity. Although Zufferey comments on the popularity of the cult, especially among the warrior nobility in Germany in the tenth and eleventh centuries, he does not pursue the point adequately. A comparison with the cults of such soldier saints as Martin of Tours or George would have been useful. Also helpful would have been comparisons of this house with such places as Novalesa or San Michele della Chiusa, other important centers located near Alpine passes through which pilgrims moved on their way to Rome and the Holy Land. These depots between the Germanic north and the Mediterranean south deserve much more attention than they have received.

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GERARD GIORDANENGO. *Le Droit féodal dans les pays de droit écrit: L'Exemple de la Provence et du Dauphiné, XXI^e à début XIV^e siècle.* (Bibliothèques des Ecoles françaises D'Athènes et de Rome, number 266.) Rome: Ecole Française. 1988. Pp. xvi, 331.

In this monograph, Gérard Giordanengo considers the development of feudal ties, and eventually of feudal institutions, in two southern French provinces between the eleventh and fourteenth centuries. He writes in full awareness of the scholarly literature on feudalism of the last two decades, American as well as European: Elizabeth Brown and Thomas Bisson are two names that figure frequently in his discussion. Giordanengo's main conclusions bear out much of the general direction of that literature. He finds that, until the middle of the twelfth century, the vocabulary used to describe

relationships we might see as feudal remained remarkably fluid. And this is not simply a trick of language. The character of the relationships themselves fell into no simple pattern, with obligations and oaths for a long time varying almost on a case-by-case basis and even afterward playing significantly different roles in Provence and Dauphiné, the two regions under study.

What gives Giordanengo's work special interest is his attention to the interaction of a learned feudal law constructed by school-trained jurists with the feudal practices of the region. The region's proximity to Italy meant that this law arrived early: Giordanengo cites a charter of 1153 of the count of Provence that makes reference to *equitas*, a legal concept then under discussion among the masters of Bologna (p. xiii). More specific references had to wait until the early thirteenth century when citations to the *Libri feudorum* began to appear, but even then the effects of the learned law depended much on circumstance. Thus, the counts of Provence did not always invoke common concepts such as that of the *forma fidelitatis*, even when it might seem to us to have aided their authority, while in Dauphiné vassals turned the law to their advantage by turning the giving of homage into evidence of nobility.

In addition to its valuable detailed account of local practices, Giordanengo's work also suggests the possibility of further work on the jurists of southern France. Although the *Exceptiones Petri* and its family of works, dating from the mid-twelfth century at the latest and possibly much earlier, are often attributed to Provence, the meager appearance in the region of learned law of any kind casts much doubt on this provenance as does Giordanengo's assertion that most Provençal jurists had to go to Italy to receive formal training. One could also ask how thirteenth-century jurists coped with the practices so at variance with their books: did they discuss their texts in isolation or did they try to reconcile them with local customs? If Giordanengo does not attempt to take us far toward answering such questions, there is still merit in posing the problem.

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STEPHEN D. WHITE. *Custom, Kinship, and Gifts to Saints: The Laudatio Parentum in Western France, 1050–1150.* (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1988. Pp. xiv, 313. \$29.95.

In this book Stephen D. White analyzes a familiar feature of medieval charters, the *laudatio parentum* or consent given by relatives to alienations of land, a topic so much discussed in earlier scholarship that one might have thought that the subject had been exhausted. White's reexamination of the practice sheds new light on some of the most important elements in the study of the French nobility of the eleventh and twelfth centuries, in particular, the aristocrats' relation to their property, the organization of their kinship groups, and their notion of their religious needs. White's scholar-

ship is impeccable: meticulous, imaginative, probing. His findings and hypotheses will influence all future work in the field.

The study is based on charters from five monasteries in western France: Saint Aubin of Angers, Marmoutiers, Saint Mary of Noyers, La Trinité of Vendôme, and Saint Vincent of Le Mans. Its focus is on the period from which the documentation is best—roughly 1050 to 1150—but in fact its coverage begins somewhat earlier than that and extends into the thirteenth century.

The scope and intensity of the work are suggested by some of the key questions that White poses: Why did the *laudatio* appear? Which persons gave it? Why were they asked for it? Why did they agree to give it? Was their consent necessary or merely desirable? Why, later, did the *laudatio* cease to be used? Through his examination of those issues, White brings into relief an integrated system in which the alienations of land no longer appear as discrete conveyances but as “complex social transactions” (p. 4) or “phases in ongoing processes of social interaction and exchange” (p. 10). For one is dealing not merely with lay patronage of monasteries but with a number of interrelationships both within the kinship group and between the donors on the one hand and the monks on the other. In return for their donations, the lay alienors received various sorts of counter gifts: spiritual benefits for themselves, for at least some of their living relatives, and for their ancestors; prestige and alliances; monastic placement for members of their families; burial rights; or perhaps material counter gifts. The present, the past, and the future were all involved. The donations helped to procure salvation for the ancestors (thus fulfilling an obligation of the heirs to those from whom their property had come) and also for the donors. Yet the permanence of the alienation, on which its efficacy hinged, was dependent on the good will of other living or even unborn relatives, whose challenge of the alienations could nullify the relationship established by the benefaction. The participation of the *laudatores* was both an attempt to stabilize the exchange and a means of associating those persons in the fulfillment of obligations and in the benefits that the transaction produced.

It is impossible to do justice here to the richness of this book. In particular, I can only note White's clarification of the legal difficulties regarding the alienation of land in the eleventh and twelfth centuries and his discussion of the choice of *laudatores* as it compares to other evidence regarding the composition of family groups. In regard to such groups, his analysis is a corrective to the sometimes exaggerated stress on agnatic kinship suggested by studies based primarily on the genealogical literature or the successional structures of the medieval nobility. More such work is needed.

White's discussion of these issues and others for the eleventh and twelfth centuries is magisterial. Readers may question whether his more tentative comments

about the discontinuation of the *laudatio* in the thirteenth century adequately explain the end—or transformation—of the earlier system of lay-monastic exchanges. That problem pertains, however, to materials outside the chronological focus of this study. It is one of numerous areas in which one may hope that others will pursue the paths that White's study has so ably opened.

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MANLIO BELLOMO, editor. *Scuole diritto e società nel Mezzogiorno medievale d'Italia*. Volume 2. (Studi e ricerche dei “quaderni catanesi,” number 8.) Catania: Tringale. 1987. Pp. 246.

The papers of a conference organized by Manlio Bellomo and devoted to the history of law in southern Italy are published in this book. Historians have concentrated their efforts on the law schools and jurists of the north, but, during the past twenty years, Bellomo and his students have redressed the relative neglect of the rich and complicated legal tradition of the Regno and Sicily. In this volume almost every aspect of law in the south is touched on. Stephan Kuttner discusses the southern canonists from the twelfth to the fifteenth centuries. Federico Martino describes the *additiones* added to the margins of a codex manuscript (E.29.inf.), now preserved in Milan's Ambrosiana, by Neapolitan jurists of the thirteenth and fourteenth centuries. Using the letters of Pope Gregory the Great, André Guillou examines the Byzantine administration of Italy and Sicily at the end of the sixth century. Guglielmo Cavallo assembles a group of Greek legal manuscripts dating from the tenth, eleventh, and twelfth centuries and written in the south that attest to the continuing influence of Byzantine law. The role of the notary in Italian society is of crucial importance for the development of law throughout Italy. Alessandro Pratesi sheds much light on this institution from the tenth to the thirteenth centuries. Finally, Horst Enzensberger analyzes “juridical culture” and administration of Norman Sicily.

There is much of value in these essays. Kuttner points out that the biographies of the jurists who contributed so much to medieval thought are not well documented, and, worse, their lives are often made fanciful by spurious, inaccurate facts. In legal history, a jurist's life, teaching career, and works have to be patched together from a wide variety of sources and a careful examination of the manuscripts. Kuttner demonstrates that this is true even for the late canonists whose works have numerous printed editions. Nicholas of Tedeschi (Panormitanus) wrote several different recensions of his *Lecture* on the Decretals of Gregory IX, but the fifteenth-century editors printed only the last. Historians who wish to trace the development of his thought as abbot, bishop, jurist, and conciliarist must sort out these traditions.

Martino also uses manuscripts to expand our knowledge of law in Naples. In the late thirteenth century, a literary genre became popular in the schools that the jurists named *additiones*, *suppletiones*, or *apostillae*. These consisted of additions made to the Ordinary Gloss of Roman and canon law. Some of the most creative and important commentaries of the late thirteenth and early fourteenth centuries belong to this genre and, because none of them was printed, are almost unknown, even to specialists. Manuscripts of both laws frequently have signed portions of these *additiones* added to their margins. Martino had already examined the *suppletiones* of Guido of Suzzara, the most important southern Italian jurist of the thirteenth century, in an earlier work. In this essay he studies the rich, additional material in the Ambrosian manuscript. He notes familiar names, such as Guido, Martinus of Fano, and Roffredus, and others who are not, such as Niccolò Rufolo, Giovanni Vacca, and Johannes de Alando.

Pratesi outlines—and given the evidence only an outline can be given—the activity of notaries in the south. The records pullulate with references to notaries in the ninth, tenth, and eleventh centuries, and the texts sometimes offer tantalizing clues about their functions and learning. Pratesi, for example, draws attention to a document of 899 that refers to a certain widow and orphan, Teopera, in Salerno, who had taken the religious habit in her own home and who wanted to leave her possessions to the monastery of San Massimo. The notaries and jurists (*iuris periti*) of the city objected that a religious woman could leave only one-third of her goods to the church; her *munduoldi* had a claim to the other two-thirds. It was decided that the palace possessed the *mundium* of those who had no parents. Pratesi cites the text as an example of how the *opiniones* of notaries and jurists were given equal weight; one might also note that the text is an interesting example of abstract legal arguments being applied to specific cases. All notaries were not, of course, learned in law, but Pratesi illustrates how much can be teased out of the thin evidence that we have.

Bellomo is a scholar's scholar. In an age when some authors and presses seem to think that indexes mar a book's final pages, he has provided the book with three extraordinarily useful indexes for this volume and its predecessor. There is an index of authors of secondary works cited, manuscripts referred to, and names. They not only are useful but also bring unity to the volumes.

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RONALD G. WITT. *Hercules at the Crossroads: The Life, Works, and Thought of Coluccio Salutati*. (Duke Monographs in Medieval and Renaissance Studies, number 6.) Durham: Duke University Press. 1983. Pp. xvi, 455. \$46.50.

In an eloquent passage of his treatise *De seculo et religione* (1381–82), the Florentine chancellor and clas-

sical scholar Coluccio Salutati described his adopted city. Like a good humanist, he offered a dramatic view, inviting the reader to climb with him the hill of San Miniato or the double mountain of Fiesole and look down at the "starry towers" of Florence's great buildings (edition edited by B. L. Ullman [1957], pp. 60–61). Like a good humanist, he stressed the grandeur of the cathedral and the Palazzo della Signoria. And then—not like a humanist at all, some would say—he deftly subverted the reader's expectations, calling attention to the fragility of those apparently solid structures. The Palazzo della Signoria was settling; the cathedral was on the verge of collapse. Where private houses had once stood, ruins and vacant lots gave mute testimony to the destruction done by the Ciompi and their enemies. Salutati's vision makes good sense in the context of his book, which was a sustained attack on pagan culture and the active life. But that vision seems puzzling in the larger context of Salutati's life. Salutati, a great public figure and pioneering classical scholar, loved Florence deeply and defended the city's material and political interests throughout his long career as chancellor of the republic. How is the historian to make sense of this man of contradictory ideals and visions?

Since the beginning of this century, scholarship on Salutati has proliferated. Francesco Novati and B. L. Ullman have edited his letters and treatises and have assembled a vast fund of information about his career as a scholar. Many intellectual historians—notably Alfred von Martin, Hans Baron, Eugenio Garin, Charles Trinkaus, and Eckhard Kessler—have analyzed his ideas about the active life and the value of classical culture. Historians of Florentine society, government, and political theory, such as Nicolai Rubinstein, Peter Herde, and Gene Brucker, have dwelt on Salutati's public writing in the service of the state. And serious discussions of his thought and impact have cropped up across the whole field of late medieval and Renaissance cultural history, from poetics to political theory.

The profusion of monographic studies, however, has often done more to confuse than to clarify the central issues. Scholars who have confronted Salutati through only one of his texts or in only one of his capacities have tended to force him into some well-defined category. They have identified Salutati as a great defender of classical learning who discovered the Roman origins of Florence or as a great preserver of medieval tradition who defended Dante's view about the historical necessity of Caesar's founding of the Roman Empire; as a proponent of the active life of the citizen or as a believer in the contemplative life of the monk; as a believer in the ability of pagan poets to discern through inspiration the truths of Christian theology or as a sharp critic of paganism who insisted on the superiority of Christianity to even the best products of classical culture. And in every case, they have found considerable evidence in Salutati's writings to support their view. The disparities in Salutati's thought have frustrated many scholars, driving them to desperate expedients in their search for a formula that would account

for his ability to have entertained contradictory ideas and still have functioned. Some, such as von Martin and Ullman, have seen him as a schizophrenic creature poised between medieval and Renaissance ways of thought and unable to choose definitively. Others, such as Kantorowicz and Herde, have treated him as a hollow man, a hired pen willing to make a case for any salable idea if doing so could serve his own ambitions. Hans Baron, in his justly famed work, *The Crisis of the Early Italian Renaissance* (1966), has treated Salutati as the Moses of the humanists, who was able to glimpse, but not cross into, the new land of republican classicism that his younger followers, inspired by the defeat of Giangaleazzo Visconti, would settle and cultivate.

Ronald G. Witt has studied Salutati for more than twenty years. Witt's preliminary publications have shed a brilliant new light on Salutati's public work as a notary and chancellor and have illuminated many aspects of his philosophy and scholarship as well. This book offers a detailed, thoughtful, and convincing account of Salutati's political and intellectual careers. Witt follows his hero up the ladder of professional success from Salutati's small native town in the Valdinievole to his great position as chancellor—something like foreign secretary—of Florence, a post Salutati held for thirty dramatic years. Witt's careful analysis of Salutati's life in office valuably supplements and corrects recent treatments of the Florentine state, which have overemphasized its modern, bureaucratic side and ignored its amateur, patrimonial qualities. And his reconstruction of Salutati's intellectual biography takes into account the books Salutati read as well as those he wrote, tracing in loving detail the impact new texts and experiences had on him and showing that many apparent contradictions in his thought disappear when the relevant passages are read in textual and biographical context. Witt's discussion of Salutati's vision of Florence in the *De seculo et religione* is only one among many insightful explications of texts.

This rich, clear, and fair-minded book is a model of its genre. Witt's admiration and sympathy for Salutati are evident throughout, and he makes a powerful case for Salutati's originality as textual critic and historical thinker. But he readily admits the rambling and sometimes genuinely contradictory nature of Salutati's discussions of the ancients and his contemporaries, and he clearly shows the interaction of private ambition with public service in Salutati's official career. Witt's book, with its vastly informative footnotes, its judicious guidance to secondary works, and its ample, unhurried treatment of every vexing question, will be a standard work of reference and a source of provocation for decades to come.

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GERALD W. DAY. *Genoa's Response to Byzantium, 1155–1204: Commercial Expansion and Factionalism in a Medie-*

val City. Urbana: University of Illinois Press. 1988. Pp. x, 196. \$21.95.

Gerald W. Day confined this study of medieval Genoa to the years from 1155 to 1204 and to the city's relations with one overseas power, Byzantium. Day believes that Genoa's chronic factionalism and internal development resulted from the twists and turns of its economic and diplomatic ties to Constantinople. This assumption does not rest on a close analysis of other important Genoese markets such as Provence, the Norman kingdom in southern Italy, Egypt, and the Crusader states in the Eastern Mediterranean. Genoese merchants had an expansive view of their commercial self-interest. Byzantium was one of a number of possible opportunities, and, hence, a sharp focus on the bilateral relationship runs the risk of distorting Genoa's political development and foreign policy.

The great merchant families who dominated the commune and Genoa's overseas commerce have left behind, in the notarial cartularies, the city chronicle, and the diplomatic records, traces of their interests and activities. Day has carefully mined these sources in order to reveal the ties among the leading families whom Day calls "the feudally minded Genoese aristocracy" (p. 9). Day singles out one leading figure, Ingone della Volta, who, according to the author, "ruled twelfth-century Genoa like the Florentine de Medici of a later period" (p. 120). This last opinion is likely to strike Florentine and Genoese historians as far-fetched. Day might have looked more closely at other Italian ports such as Pisa and Venice for some contemporary comparisons about how the nobility took to the sea.

The notarial records, most importantly the published cartularies of Giovanni Scriba, Oberto Scriba de Mercato, and Guglielmo Cassinese, provide most of Day's evidence on the links between commerce and foreign policy. The bibliography indicates that Day has conflated two separate collections of notarial records in the Archivio di Stato of Genoa, and so he has missed hundreds of relevant documents in the Notai Ignoti that might have helped him establish the importance of Genoa's trade with Byzantium in the 1190s. Day uses the commercial contracts to demonstrate how the leading families cooperated, along factional lines, in overseas trade. The actual number of contracts concerning Byzantium, however, forces Day to conclude that "the testimony of the notarial evidence for Genoese trade with the Byzantine Empire in the twelfth century is then virtually silent" (p. 163). This unduly pessimistic view finds a corrective in David Abulafia's *The Two Italies: Economic Relations between the Norman Kingdom of Sicily and the Northern Communes* (1977) in which Abulafia analyzed the range of sources for 1191 and found a considerable role for Genoese trade with Constantinople (p. 182).

Because Day did not examine closely the issues of trade, commodities, and markets, his analysis of Genoese political factions relies heavily on the narrative sources and diplomatic records. Day has put together

an interesting and detailed portrait that reveals how factions jockeyed for power in the commune. The year 1204 provides a natural terminus for Day's study: the Fourth Crusade seized Constantinople, and the Genoese found themselves excluded from this portion of the eastern trade. As the Genoese looked East, their longstanding ties to Egypt and the Crusader states enabled them to adjust their sights. Day's book is a good point of departure for scholars looking at the broad range of influences on the origins of the Italian trading powers.

STEVEN EPSTEIN
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DAVID NICHOLAS. *The van Arteveldes of Ghent: The Varieties of Vendetta and the Hero in History*. Ithaca: Cornell University Press. 1988. Pp. xx, 212. \$23.50.

James van Artevelde (1290?–1345) and his son Philip (1340–1382) ruled the city of Ghent and almost the whole county of Flanders during two of the most tumultuous periods in their history. The elder van Artevelde assumed the chief captaincy of Ghent in 1338, when Flanders was in the grip of a devastating economic crisis. During seven years in office, van Artevelde, through a farsighted foreign policy, restored a measure of prosperity. He championed the Flemish "national" interest against the Francophile count Louis II de Nevers and his allies among the landed nobility and the urban patriciate. Philip van Artevelde acceded to the chief captaincy in January 1382, at the height of a political conflict that opposed Ghent to Louis de Male, the son and successor of Louis de Nevers, who sought to bring that city under his control. Rash and impolitic, Philip provoked his own demise less than one year after taking office.

Since the fourteenth century, the reputation of the van Arteveldes has undergone a remarkable metamorphosis. Scorned in their own time by the upper orders as vulgar upstarts and rebels against the lawful sovereign, they were ignored and largely forgotten until the early 1800s. The national awakening of Flanders in the nineteenth century combined with Romantic hero worship to produce a gradual rehabilitation of the two. James and Philip van Artevelde became identified in public opinion with the struggle of the Flemish people for emancipation from the Walloon hegemony in Belgium. Although the advent of professional history in the 1880s dissipated some of the enthusiasm, the new scholarship nonetheless retained and perpetuated their image as pioneers of Flamingantism and democracy.

This book does not add greatly to our knowledge of James van Artevelde's life because earlier historians have left little to uncover. Nevertheless, David Nicholas's deft and imaginative handling of the camera accounts of the Ghent aldermen has enabled him to correct and amplify the elder van Artevelde's biography on a number of points, notably the circumstances surrounding his death. The book's chief contribution is

to the life of Philip van Artevelde, who has not been the subject of monographic study. Nicholas is concerned less with the political role of the van Arteveldes than with the pattern of violence that marked their public careers. He stresses that aspect of their lives against the backdrop of inveterate hatreds and internecine feuds in Ghent family clans. By focusing on violence, the author parts company with scholars such as Léon Vanderkindere, Henri Pirenne, Henry S. Lucas, Hans van Werveke, Paul Rogghé, and Patricia Carson.

For all of Nicholas's awesome erudition, his work is flawed in several respects. He takes a highly prejudiced view of medieval violence—one that reenacts the moralistic stance of the Enlightenment and that also distorts his judgment of the van Arteveldes' place in history. He asserts that father and son sought power in order to eliminate personal enemies and settle private scores as well as to consummate their passion for self-aggrandizement. He portrays them as harsh and arrogant men, who lacked any of the attributes associated with democratic leadership.

It is difficult to reconcile the author's view of James van Artevelde with what is known about his life. If selfish ambition had been his primary motivation, as Nicholas contends, van Artevelde would doubtless have tried to institutionalize his power in 1340 by claiming the regency of Flanders for himself, instead of awarding it to a political ally. And if he was often harsh and arrogant, his behavior was not appreciably different from that of most other public men in that age. In any event, it is an incontrovertible fact that James van Artevelde brought all the craft guilds into the Ghent government in 1338, including the revolutionary weavers, and that he reestablished the annual stipend for the aldermen. Nicholas's charge that van Artevelde did nothing to alleviate the plight of the Ghent journeymen, on account of his alleged failure to overturn the monopolization of the guild masterships, is unjustified because that development did not take place, at any rate among the dominant textile crafts, until the second half of the fourteenth century. It is thus hard to understand Nicholas's rejection of James van Artevelde as a precursor of Flemish democracy. His portrayal of Philip van Artevelde is even more disparaging. The chapter on Philip's youth is titled "'Filthy Phil,'" although that epithet apparently originated only after his death with his enemies at Bruges and was merely a slur, as Nicholas is well aware. Another chapter is titled "The Hero Repugnant," despite the fact that the common people of Ghent hailed the younger van Artevelde as their savior. The author does not hesitate to dub Philip "the archetypal godfather" and so assimilate him to the head of a modern crime syndicate, even though he knows better than anyone that avenging the murder of his sire was a sacred obligation for an oldest living son in fourteenth-century Ghent.

I am truly baffled by this radical "deconstruction" of one of the outstanding figures in the history of medieval Flanders. I am further perplexed by the book's ubiquitous bias, by its pervasive reductionism, its excess

of special pleading, and its all too obtrusive tendency to judge the past from the exclusive purview of the present.

PAUL ROSENFELD
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FRANTIĚK GRAUS. *Pest-Geissler-Judenmorde: Das 14. Jahrhundert als Krisenzeit*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 86.) Göttingen: Vandenhoeck und Ruprecht. 1987. Pp. 608. DM 122.

It has been over a century since Robert Hoeniger, in *Der schwarze Tod in Deutschland* (1882), first established a chronology of the progress of the plague in Germany during the late 1340s. That study declared that the pogroms and flagellant processions that swept Germany during the period reached their climax well before the disease entered German-speaking Central Europe. Heoniger's work has stood the test of time among German-speaking scholars, and it is a damning comment that his findings remain unknown among historians whose primary language is French or English. František Graus has confirmed Hoeniger's work and given it unexpected depth and resonance.

Graus portrays the epiphenomena of the Black Death as a "Great Fear" crisis that flashed across Germany, divorced from the slowly advancing front of the Black Death in Switzerland. In Germany, then, one is dealing with a mass movement that did not rely on the health of rats and fleas. The rumor of well poisoning, which had appeared in southern France much earlier, was carried by both official messengers and word of mouth to communities in which there was a precarious balance between competing sociopolitical groups. The resulting massacres of Jews, which were usually carried out in cold blood and often after consciously staged trials, constituted the most important single spasm of pogrom in Germany between the First Crusade and the obliteration of Central European Jewry in the twentieth century. Graus has endeavored to detach those massacres from the Yom Kippur tradition, in which each pogrom looks rather like the last, and he has placed them firmly in the tradition of fourteenth- and fifteenth-century Western European social and political unrest.

Graus moves from his consideration of pogroms and flagellants during 1348–49 to an analysis of the nature of crisis and rebellion in late medieval Europe. Here Graus shows that he has moved beyond the Marxist interpretations of earlier years to something much more complex and nuanced. His own research on the Hussite revolution has helped give his work on the Black Death greater scope. To him, the rebellions of the later Middle Ages were both rural and urban, and their degree of self-consciousness was higher than usually believed. In the case of the massacres of the 1340s, the "mob" was only blamed for the killings after

the fact, when it suited the elites to argue that the destruction had been caused by an irrational mass response to the plague. Just as is the case with the English rising in the 1380s, the truly frightening thing about late medieval popular risings was not their irrationality but their ideological clear-sightedness. Their goals were worse than utopian: they were attainable.

Graus makes a thematic statement: "But one should finally put a stop to the widespread mischief of portraying the past in such a way that people only begin to think in the second half of the eighteenth century, with a few exceptions (in extreme cases only in the second half of the twentieth century)" (p. 511). It is a relief to read such a wise and calm book about such a terrible subject.

STEVEN ROWAN
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St. Louis

MICHAEL H. SHANK. *"Unless You Believe, You Shall Not Understand": Logic, University, and Society in Late Medieval Vienna*. Princeton: Princeton University Press. 1988. Pp. xvii, 257. \$35.00.

This book is a study of the early intellectual history of Vienna University from the 1380s to the beginning of the 1420s. Its main theme is the later medieval debate over the relation of Aristotle's logic of the syllogism to the Christian doctrine of the Trinity. Michael H. Shank traces the development of that debate from Duns Scotus, who first stated it as a logical problem and sought to resolve it in its fourteenth-century form, to the eventual rejection of any solution by Henry of Langenstein shortly before his death in Vienna in 1397. The last part of the book examines the implications of that rejection as they concerned both Viennese society, above all, Langenstein's involvement with the large and still well-established Jewish community there, and wider changes in Christian attitudes toward infidels and heretics that Shank discerns by the time of the Council of Constance and its aftermath in the Hussite revolt. The link between the earlier intellectual issue of the accessibility of God's triune nature to logical demonstration and those later practical considerations was provided by the migration of Langenstein and his friend and fellow theologian Henry of Oyta to Vienna from Paris University as part of the secession of the German and other foreign masters and students from France following the outbreak of the Great Schism. They brought with them to Vienna the issues current in Paris, including the problem of the logical status of the Trinity. At that time Langenstein, like Oyta, shared the prevailing scholastic view that Aristotle's logic was universally valid for all that came under natural reason but that the mysteries of faith, and none more than the nature of the Trinity, lay beyond natural reason and so beyond logical demonstration. Langenstein's break with that consensus consisted in coming finally to deny the universality of Aristotelian logic. Shank persua-

sively argues that the cause of Langenstein's change of mind was the outcome of his involvement with the Jewish community at Vienna and his failure to convert them to Christian beliefs. That failure undermined his unqualified belief in Aristotelian logic. His loss of conviction had a powerful effect on his students on the Vienna theological faculty and helped undermine the more relaxed and open attitude toward intellectual difference that existed in the 1380s.

A brief summary cannot do justice to the breadth and depth of Shank's scholarship. Although he draws on previous work, scrupulously acknowledged, for his account of the main scholastic doctrines of the relation of logic to the Trinity until the time of Langenstein and Oyta, their subsequent development at Vienna and their place in the larger religious context of dissent and reform in the early fifteenth century represent an original and important contribution to the intellectual history of the later Middle Ages that will help bring a new perspective to the period.

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MODERN EUROPE

V. G. KIERNAN. *The Duel in European History: Honour and the Reign of Aristocracy*. New York: Oxford University Press. 1988. Pp. viii, 348. \$49.95.

V. G. Kiernan studies the duel in European history from its hoary beginnings in the ritualized encounters of primitive man until the present. Primitive man, he finds, was "not over-vindictive. To brutalize him further, nature had to be reinforced by social advances, division into higher and lower classes as well as into kinships" (p. 23). "Duelling and its code of honour . . . came to form a powerful link between all noble ranks, and 'strengthen[ed] their sense of belonging to a single privileged class'" (p. 52). To Kiernan, dueling was thus an instrument for developing a class consciousness among its upper-class practitioners. Although falling behind other classes in education and intelligence, the military section of the upper class "pinned their faith to 'character,' most easily defined as ability to face shot and shell, or the duelling sabre or pistol, undaunted. It was a philosophy not without practical returns, over a long period. Small European forces conquered a great part of the world; what mattered much more, almost all attempts at revolution inside Europe were crushed. As something that helped to build up the morale which made these achievements possible, the duel is entitled to its share in their halo" (p. 115). Women made no concerted effort to outlaw the duel. They were raised to believe that dueling was "an inescapable duty of their men-folk, like going to the wars, or like childbirth in their own case" (p. 13).

After briefly treating the ancient and primitive analogies to dueling, the joust, and the trial by battle of the medieval knight, Kiernan finds the origins of the

modern duel in Renaissance Italy. From here it spread to other parts of Europe, reaching its peak roughly between 1550 and 1750. Although the Enlightenment undermined the rationale of the practice and the French revolution condemned "everything feudal or aristocratical, the ensuing twenty years of European war seemed to revive and reinvigorate it, as though with the smell of fresh blood. It lingered on in Britain to near the middle of the nineteenth century, on the continent until the deluge of the Great War made bloodshed over petty private grudges meaningless. Meanwhile it had been carried overseas, especially to the Americas, by the expansion of Europe" (p. 7).

To document his interpretation of the duel as an instrument to develop upper-class consciousness, to provide the inspiration to suppress internal revolutionary movements, and to subject Third World peoples, Kiernan provides the reader with hundreds of accounts of duels as recorded in letters, memoirs, and works of fiction. His practice of limiting each episode to a sentence or two often leaves the reader in doubt about the background of the protagonists or the long-ranged outcome of the fracas. No real effort at quantification is made, and the reader is never sure how widespread the practice of dueling was. Although the author's Marxist interpretation lends itself to social history, his contempt for "an obsolescent and for the most purposes useless class" (p. 152) prevents him from painting a convincing picture of the duelist's social and intellectual mores. The practice had its fatal attraction, which penetrated into the middle class, to the German university student, and to the common soldier, and in rare instances it had its women practitioners. Even Karl Marx and Friedrich Engels did not escape the ethos. Thus, Kiernan's study falls short of what is desired, but it is full of lively anecdotes and caustic phrases that capture the reader's attention in spite of wild exaggerations, for example, when he tells us that Americans are "likely to have pistols in their pockets all the time" (p. 145) and that "every American city is now a Wild West" (p. 314).

RUSSELL MAJOR
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MYRON P. GUTMANN. *Toward the Modern Economy: Early Industry in Europe, 1500-1800*. Philadelphia: Temple University Press. 1988. Pp. xiii, 257. \$29.95.

With the publication of Franklin Mendels's "Proto-Industrialization: The First Phase of the Industrialization Process" (*Journal of Economic History* [1972]), the attention of many economic historians shifted from urban to rural development and from industrial revolution to economic evolution. Nowhere is the impact of Mendels's work more in evidence than in Myron Gutmann's book. Sharing Mendels's interests in economic and demographic history and adding his own concern with attitudes and social structure, Gutmann emphasizes both the gradual nature of European economic

change and the rise of large-scale cottage industry. The book is not a study of nineteenth-century proto-industrialization, however. Gutmann's analysis spans five centuries, 1300–1800 (focusing primarily on the last three of the five), and covers three crises or turning points in European economic development—the late fifteenth- and early sixteenth-century decline of established urban industrial centers when merchants failed to adapt quickly to changing political and economic circumstances, the death of guild-controlled urban industries as production shifted from urban workshops to rural cottages in the seventeenth century, and the return of manufacturing to urban areas in the eighteenth century as production was again centralized and then mechanized.

Despite his references to crises and turning points, Gutmann emphasizes continuities and the incremental nature of economic development. Indeed, his central argument is that “industrial growth and change was a logical and evolutionary process, undertaken by businessmen from an older milieu who were in search of solutions to immediate problems” (p. 232). Gutmann's analysis of the problems to which large and small entrepreneurs, merchants, and artisans responded includes rising and falling population levels; shifts in political power and tariff barriers; state (and especially military) demand for products; the merchants' access to and control of raw materials; technological developments that by themselves could alter the location of industries (fulling mills, for instance, required swiftly running water, and the operation was moved out of the textile towns and into rural villages); shifts in the demand for agricultural labor as regions shifted from one type of cultivation to another; and competition from new products that could and often did eliminate demand for older goods.

The value of Gutmann's work lies in his linkage of social, economic, and demographic change. As he observes, the availability of workers and consumers, the level of agricultural and industrial production, and the organization of production are intimately related. A change in one is virtually impossible without a change in the others. Gutmann is particularly good at explaining the demographic changes that occurred in early modern Europe in language that is readily accessible to nonspecialists and is strongest in his analysis of the shifting fortunes of the wool industries in the Low Countries.

The most awkward aspect of the book is its hybrid nature, a term that Gutmann himself uses to describe eighteenth-century society. The volume is both a survey of the general pattern of European economic development before the Industrial Revolution and a case study of the textile industry in the southern Low Countries. The virtue of such an approach is that the general theory is concretely illustrated. The drawback is that it provides little or no information about industries in other regions. Gutmann may have integrated his own research with a historical synthesis (which is the avowed goal of this series [p. ix]) about as well as

anyone could, but I am left wondering whether such a combination is, in fact, workable. A synthesis that gave equal weight to a wide variety of regions, explaining why they did or did not follow the general pattern; a history of economic development in the Low Countries examining a variety of industries and agriculture in approximately equal depth; or a general history of the European textile industry, all subjects about which Gutmann possesses considerable knowledge, would have allowed him to develop the same gradualist argument and might have made more satisfying reading.

Nevertheless, there are many reasons to recommend this work: persuasive argument, new information about the Flemish and Liègeois textile industries, insightful explanations of demographic behavior patterns, and a well-documented challenge to the Anglo-centric view of industrial development. In short, the book adds considerably to our understanding of early modern industrial development and should be read by scholars and teachers in the field.

GAY L. GULLICKSON
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PIERRE BIRNBAUM. *States and Collective Action: The European Experience*. New York: Cambridge University Press. 1988. Pp. 232. \$39.50.

Pierre Birnbaum's new book consists of a collection of extracts from four previously published works, which are here newly translated. In the introduction to the volume, the author states his purpose as follows: “Instead of explaining the type of state in terms of a particular arrangement of variables, I shall treat it as a variable which will serve to account for the behavior of groups, parties, classes and nationalist movements. . . . The logic of each state influences the mode of action of actors in ‘civil society,’” and that logic is the outgrowth of antecedents, here left unexamined, which have created different ideal types of state eventuating in the “European experience” since the end of the eighteenth century.

In terms of these agenda, the several chapters deal in turn with a general analysis of collective movements; the mobilization for war during the French revolution; a comparative analysis of Germany, England, and France in terms of the relation between state structures and individual or collective action, ideologies, and the labor movement; the problem of corporatism in France and England; the conflict between the Nazi movement and the Prussian state; ethnic mobilization in Scotland, Brittany, and Catalonia; national and cultural aspects of the Zionist movement; and, finally, the West-Indian movement in relation to the state in England. There are nuggets to be found in this array of topics; the significance of military mobilization during the French revolution is clearly delineated as is the antistate tendency of the Nazi movement. I found the discussion of ethnic mobilization in several chapters (including the one on Zionism) worthwhile, because it is a topic of

major interest in recent history and will be, I suspect, in the years to come.

But having said this, I come away from the volume with unanswered questions, even though I am in full sympathy with Birnbaum's objectives announced in the introduction. He restricts the variety of state structures that he examines to the European experience. Good. He abandons the reductionist perspective that would treat the state as a variable dependent on class, party, and so forth, and claims for its historically developed structure considerable, independent significance. Good again. He opposes the causal to the ideal-typical approach and opts for the latter, which appears appropriate in view of the complex materials involved. But why then talk about the state as a variable, when no attempt is, and probably cannot be, made to deal with "the state" in terms of separable, measurable dimensions? And, if the objective is to vindicate the independent significance of the state, why not restrict the discussion to pairs of comparison that are easier to handle, such as the different military structures in France and Prussia or the effect of British and French state structures on the organization of the police? As it is, many of the discussions assume more detailed knowledge than many readers have and move too quickly from topic to topic to carry conviction.

Comparative studies are hard to write, because authors must choose between a case-study approach with comparisons coming at periodic intervals and more frequent comparisons at the expense of coherence of subject matter. If the present volume has not resolved this dilemma successfully, then this may be because its parts are derived from several independent, original publications.

REINHARD BENDIX
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M. S. ANDERSON. *War and Society in Europe of the Old Regime, 1618-1789*. New York: St. Martin's. 1988. Pp. 239. \$35.00.

This book is the latest volume in the Fontana "War and Society" series edited by Geoffrey Best. Inspired by a growing literature on the subject, these works seek to describe the impact of war on society and the ways in which social and economic structures have influenced the conduct of war. Battlefield history is not emphasized, but the topic remains large enough to touch on virtually every aspect of human activity. By attempting to cover the entire period from the Thirty Years' War to the French revolution, M. S. Anderson has set himself a formidable task.

Between 1618 and 1789, two great movements altered the conduct of European warfare. The first was the extension of state control over military affairs to a degree unheard of in earlier times. Inspired by new conceptions of sovereignty and by the debacle of the Thirty Years' War, this transformation was largely

complete by 1700. The second development, associated with the growth of Prussia and above all with revolutionary France, was the attempt to mobilize entire societies in the service of war. Both movements involved changes in strategy, tactics, organization, and finance, to say nothing of the character of governance itself. Each was both the reflection and the cause of major alterations in mentality and social life, and either could legitimately be the subject of a book far longer than this one.

To deal with so much in only seventy-five thousand words requires prodigious feats of condensation. Anderson is forced to describe the history, organization, and support of armies in at least nine countries and to relate the changes that took place in each of them to a rich variety of intellectual and political movements. Unlike some authors in the Fontana series, he deals effectively with Eastern Europe and with some of the more important aspects of war at sea. He even attempts to assess the costs and benefits of early modern warfare in the spirit of Werner Sombart and John U. Nef, although his analysis is primarily economic and questions of technological development are largely avoided.

Each of these subjects is complex and beset with semantic and ideological difficulties. Anderson asks most of the right questions and his judgments are usually sound, but there is little room for subtlety or nuance. Complicated issues involving recruitment or finance are oversimplified and sometimes difficult to follow. Possible explanations for several developments are left unexplored as are at least some of their ramifications for society. These omissions may be the result in part of limitations of space, but they also reflect the fact that "war and society" studies are in their infancy. On many subjects the critical mass of research needed to sustain conclusions simply does not exist. The author knows this and proceeds with caution, but his book as a whole retains a tentative, incomplete quality. It is perhaps best to regard this study as a preliminary introduction to a growing body of literature, a survey that points the way to further research rather than an authoritative synthesis in a mature field.

WILLIAM S. MALTBY
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MICHAEL DUFFY. *Soldiers, Sugar, and Seapower: The British Expeditions to the West Indies and the War against Revolutionary France*. New York: Clarendon Press of Oxford University Press. 1987. Pp. viii, 420. \$78.00.

Michael Duffy's purpose is to understand the importance of the Caribbean slave plantation colonies to the maritime and commercial world of the eighteenth century and to set the West Indian military expeditions of the 1790s "in their proper place in one of the most difficult and dangerous wars of British history" (p. v).

Duffy seeks to show how Britain used its seapower, colonies, and economic strength to match and eventually overcome revolutionary France with its larger population and greater number of men under arms. He says that the purpose of the war in the Caribbean was to counterbalance French military domination of the Continent with Britain's naval and commercial domination of the sea and to use the conquered French and other colonies as bargaining counters for trading off French gains in Europe at the peace conference.

Unlike historians who have regarded the Caribbean colonies as millstones around the neck of the mother country, Duffy considers them a "precarious money-box" (p. 19) where Europeans ran great risks to their health but found opportunities for economic gain that made the islands the biggest depository of British and French investment overseas. By the 1790s those investments had been multiplied and spread over the island and coastal mainland colonies of Spain, France, Britain, Holland, Denmark, and Sweden. Sugar, which had been king of the islands, was being challenged by the growing production and consumption of coffee, while cotton and several lesser products were supplying the demands of European and North American markets.

Duffy devotes approximately two-thirds of his study to the British expeditions to the West Indies and the war against revolutionary France. One of those expeditions deserves special mention. To turn the tide against France, whose revolutionary leaders had fomented slave and Maroon rebellions in the British islands, Henry Dundas, who was William Pitt's war minister, mounted the Abercromby-Christian expedition of 1795-96, "the largest single long-distance overseas expedition to depart from British shores" (p. 196). It met with innumerable bottlenecks, delays, and postponements, resulting partly from difficulties in finding commanders, manpower, warships, transport, and logistic backing; partly from bad weather and contrary winds; and partly from problems of disease and desertion. Despite those handicaps, British expeditionary forces regained the islands of St. Lucia, St. Vincent, and Grenada; occupied the Dutch South American colonies; and saved Jamaica from the rebellious Trelawny Maroons and their slave allies. On the other hand, Guadeloupe remained under French control, and British forces in Saint Domingue had to be reduced and eventually withdrawn in the face of mounting attacks by rebel blacks under Toussaint l'Ouverture. By the Treaty of Amiens of 1802, Britain surrendered all of its Caribbean conquests from France, Denmark, Sweden, and Holland in exchange for concessions in Europe, retaining only Spanish Trinidad.

Although the loss of life was horrendous, Duffy contends that the Caribbean war served its purpose in helping to establish decisive British maritime and commercial supremacy over France. Indeed, he argues that the soldiers and sailors who died in the expeditions sent to the West Indies were protecting the future of Britain itself and not that of the British West Indies.

Duffy's book is a painstaking and meticulous study that is based on a vast array of primary sources and secondary works. It is written in clear and vigorous prose and displays prodigious learning and scholarship. It is a substantial work, which the publisher's blurb tells us "is the first full-scale examination of the politics, economics, administration, and execution of the expeditions to the West Indies which were mounted by the British against the French during the Revolutionary Wars." It was awarded the Templar Medal by the Society for Army Historical Research. Duffy's book compares very favorably with Richard Pares's great work, *War and Trade in the West Indies, 1739-1763* (1936), and I highly recommend it.

RICHARD B. SHERIDAN
University of Kansas

FRANK B. TIPTON and ROBERT ALDRICH. *An Economic and Social History of Europe*. Volume 1, 1890-1939; volume 2, *From 1939 to the Present*. Baltimore: Johns Hopkins University Press. 1987. Pp. vii, 323; vii, 297. Cloth the set \$62.50, paper the set \$22.90.

Given the common preoccupation with the political history of Europe during the past hundred years, a work stressing the economic and social aspects is welcome indeed. Two Australian historians, Frank B. Tipton and Robert Aldrich, have done a diligent and professional job of reducing the overabundance of information to a manageable two-volume outline.

Presenting an outline, these volumes are inevitably limited in interpretation and selectivity. Despite the title, political history still provides the basic framework of organization. Volume 1, for instance, contains chapters on European expansion, on politics and ideology, on World War I, and on the origins of World War II. The second volume deals with World War II, European reconstruction, and, again, politics and ideology. The authors, moreover, are aware that Europe must be understood in the broader context of the world. Especially in the second volume, they carefully trace the changing role of Europe in world affairs. Throughout, however, the treatment of political history is admirably, if sometimes perhaps too densely, laced with information about economic realities and the policies designed to control them. The chapters on social history, fact-packed and wide-ranging, stand slightly apart, yet still relate to the main theme.

That theme is indicated in the introduction to volume 1: "One of the most important developments . . . was the increasingly close connection among economic, social and political events. Though at times politicians seemed prisoners of economic and social developments beyond their comprehension, European governments gradually groped their way towards more extensive and effective intervention in economic and social life. Politics became the sphere where social and economic issues overlapped" (vol. 1, p. 7). That trend is spelled out more prominently in volume 2: "The

Second World War and the reconstruction period saw the completion of the process of interpenetration of economy, society and politics which had begun before the First World War. . . . Western European governments adopted an indirect 'Keynesian' approach, while their eastern European counterparts preferred the 'Soviet' model[,] but all agreed that economic development could and should be 'planned' and that social policy could and should both foster economic development and minimize its possible negative effects" (vol. 2, p. 6). The progressive interaction of government, economics, and society furnishes proof that the quality of life everywhere in Europe improved.

As regards the post-World War II years, American readers, used to viewing postwar Europe as sharply divided between East and West, will be startled by the authors' insistence on Europe's continued unity. The generation before 1945, Tipton and Aldrich argue, failed to solve the social problems confronting their societies. By contrast, "out of the Second World War . . . emerged institutions based on a new consensus" on how to solve those problems. Admittedly, the East-West split dominated those institutions. "Nevertheless, Europe remained a unit . . . all of the European nations did address the same problems . . . all moved along the same trajectory of development" (vol. 2, p. 4). Did they really? In any case, the author's treatment of Eastern Europe, including Russia and the Soviet Union, well-informed though it is, remains somewhat perfunctory. Obviously, the emphasis lies on the major states of Western Europe, combined with outline histories of every other European country. This is an impressively fact-laden survey indeed, even if occasionally it approaches the aridness of encyclopedic summaries.

Some of the "facts," however, are wrong. Hitler, for instance, went to Germany in May 1913, not, as the authors assert, after the outbreak of war in 1914 (vol. 1, p. 295). Likewise, by most accounts his body was charred (rather than incompletely burned) before the Russians arrived at the Führerbunker in May 1945 (vol. 2, p. 46). Specialists will discover similar petty flaws in other sections. More contentious, perhaps, is the authors' emphasis on the role of political leadership. They note "the failure of the leaders of the 1920s and 1930s to solve the problems inherited from the prewar generation" (vol. 1, p. 3). Was it simply because of poor leadership that events turned awry? In addition, political theorists might find the treatment of ideology somewhat shallow. The authors approvingly quote a dissident Polish philosopher on the subject: "However grotesquely remote from reality, [ideology] is the main, indispensable instrument for legitimizing power systems" (vol. 2, p. 199). The question remains: What constitutes reality? If ideological legitimization succeeds, there must be a corresponding effective reality after all. For the authors, ideology merely serves as a convenient method of categorizing diverse political systems and schools of economic thought, without probing into the underlying realities.

The concluding chapters of the second volume,

leading up to the present, are understandably the most open to debate. How far, for instance, did "the Americanization of Europe" go? According to the authors, "Europe and America remained culturally rather distinct, and within Europe national characteristics remained strongly marked" (vol. 2, p. 198). True. But does the evidence cited here cover the entire ground? The 1980s are treated, under a facade of statistics, somewhat impressionistically. Taking a sober view, the authors write of "The Continuing Crisis" and end up with an epilogue entitled "The Third World War: An Agenda," which in this fast-moving age appears somewhat dated in tone and fact.

Considered in their entirety, however, these volumes represent a creditable achievement in bringing together a mass of information useful for all historians concerned with European history of the past hundred years. The chapters on the two world wars and on European society provide attractive assignments for undergraduate courses on modern European history; the appended maps and suggestions for further reading are additional teaching aids. Although only a survey, these volumes are a healthy reminder that historical understanding must synthesize a wide range of data. They point the way toward a more holistic grasp of the dynamics behind historical change.

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PATRIZIA DOGLIANI. *La Scuola delle reclute: L'internazionale giovanile socialista dalla fine dell'Ottocento alla prima guerra mondiale.* (Fondazione Luigi Einaudi, number 2.) Torino: Giulio Einaudi. 1983. Pp. x, 323. L. 25,000.

Patrizia Dogliani has given us the first comprehensive work on the Socialist Youth International from the end of the nineteenth century to World War I. Furthermore, in a concluding chapter entitled "The End of a History and the Beginning of a Historiography," the author summarizes the bolshevization of the Youth International's structure and history.

Scholars will welcome the appearance of this book, which fills an important gap in the history of the period and in our knowledge of socialism during the Second International. Good works on the Second International itself are scarce, and on the youth movement, even rarer. The works we do have tend to emphasize developments at the various congresses—an inevitable early stage of research in the history of European socialism. Dogliani's book, however, goes beyond congressional conflicts. In a revealing section, she provides a comprehensive description of social conditions and changes that the youth of the period faced and of the motivations that spurred young people to join the socialist youth movement. She discusses the burning issues confronting youth, especially conscription and the antimilitarist struggle. Dogliani also devotes ample space to important individuals involved with the youth movement, such as Karl Liebknecht and Henri de Man, to

the Vienna-based International Secretariat's action, and to the role of the youth organizations in the crisis leading up to and during World War I.

Her detailed and exhaustive method allows Dogliani to reach important conclusions. For example, she corrects the false impression that a concerted effort of the socialist Left, or "bolsheviks," was primarily responsible for the founding of the Socialist Youth International (pp. 55–56). Besides her conclusions, it is useful finally to understand the mechanics of the youth movement's construction, its organization, and its destiny.

The author has worked from a wide array of primary sources and secondary works. She puts the youth movement in context and provides a good review of the literature on the subject in her text. She concentrates on the Austro-Hungarian and German heartland of the socialist youth movement but considers other areas as they become more important, especially Switzerland, Scandinavia, and Italy during World War I.

If there is a weakness in this work, it is the absence of a bibliography, let alone the detailed and comprehensive one that scholars demand of such a work. In addition, although the book has a good index of proper names, it lacks a subject index. Both of those faults are common in works published by Italian publishers, and I will welcome the day when they are corrected.

I also look forward to the time when scholarship of the caliber demonstrated by Dogliani takes less than five years to reach American scholars.

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HANS A. SCHMITT, editor. *Neutral Europe between War and Revolution, 1917–23*. Charlottesville: University Press of Virginia. 1988. Pp. ix, 257. \$29.95.

Because historians tend to concentrate on the major powers, competent studies about the lesser states are often hard to find. In addressing how Europe's neutral nations responded to war and revolution between 1914 and 1923, the contributors to this volume try to fill a number of gaps. Alas, the results of a much-needed effort are very uneven and not as helpful as they might have been. Although the history of small states should not be overlooked, it should be kept in perspective, which several of the authors singularly fail to do.

Gerald H. Meaker leads off with a long, thoughtful essay about Spain's "civil war of words" between conservative Germanophiles and liberal pro-Allied thinkers, including such major figures as José Ortega y Gasset and Miguel de Unamuno. Meaker's analysis is clear, organized, and valuable to specialists and general readers alike. He ends with a brisk examination of three discrete quasi-revolutionary episodes in 1917, sensibly concluding that neutrality served the forces of order inasmuch as Spain lacked enough misery, enough involvement of the masses, and, especially, a defeat to trigger a revolution.

As one proceeds to chapters on Switzerland, the Scandinavian monarchies, and the Netherlands, the quality of the book deteriorates. The subject matter is mostly trivial, unfocused, and unexplained in essays that lack context, perspective, and point. Too often the structure is shapeless, the prose graceless, and the effect murky. Here one is given exhaustive analysis of a two-day strike that fizzled (but that somehow "seriously endangered" [p. 82] the political system); there the news that syndicalists favored birth control but no clue as to why this matters; elsewhere an unexplained declaration that attitudes toward prohibition were an indicator of radicalism (despite clear economic motives), as well as interminable dissections of election statistics for a single district whose significance is a mystery; and everywhere miscellanies of stray information, usually clumsily presented. There is much examination of small left-wing elements, but none of this detail adds up to much, and staying awake is a challenge.

The tour of the neutrals concludes with Hans A. Schmitt's chapter, "Belgium, the Dwarf States, and Luxembourg," which devotes four pages to Belgium, two and one-half to Liechtenstein, and eighteen and one-half to Luxembourg. This curious allocation of space permits a solid survey, probably the best and most accessible summary available in the English language, of Luxembourg before, during, and after World War I. Because the Grand Duchy's economic and strategic importance far outstripped its Lilliputian size, this essay meets a real need. Part of its success arises from the fact that Schmitt does not concentrate on a detailed account of riots in the capital, even though they precipitated in part a change of monarch, which is more than the exhaustively examined demonstrations elsewhere did; rather, he provides a broad survey in a larger European context. One wishes that his coauthors had followed his example.

In addition, Schmitt's chapter, like those on Spain and Holland, provides an excellent and up-to-date bibliography for the three countries he discusses. Other authors are less helpful in this respect. All chapters appear to be based almost entirely on published materials, but some are massively documented, whereas one or two are so lightly footnoted as to be virtually *ex cathedra*.

In a concluding chapter, Stephen C. MacDonald struggles to put this disparate collection of material into some sort of context and to answer the questions posed at the outset about how neutrality reduces the effects of war and to what extent the neutrals were participants and victims. After an excessively elaborate analysis, he concludes that neutrality mitigated economic misery and, above all, spared the states in question the defeat that precipitated revolution in Russia, Germany, the Habsburg empire, and, to a degree, Bulgaria. Because Germany was both centrally located and more thoroughly European than Russia, revolution there had greater influence on other states, including the neutrals, most of whom were Germany's

neighbors. German socialist leaders, however, like those of the neutrals, were not very revolutionary or energetic, so the forces of order soon prevailed among both the defeated and the less traumatized neutrals. These conclusions are sensible and provide some needed perspective, but they can hardly compensate for the inadequacies of much of what comes before.

SALLY MARKS
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MARIE-LUISE RECKER, editor. *Von der Konkurrenz zur Rivalität: Das britisch-deutsche Verhältnis in den Ländern der europäischen Peripherie 1919–1939/From Competition to Rivalry: The Anglo-German Relationship in the Countries at the European Periphery, 1919–1939.* (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 16.) Wiesbaden: Franz Steiner. 1986. Pp. 186. DM 38.

In editing this volume of four studies of Anglo-German trade rivalry, Marie-Luise Recker has made a commendable attempt to extend such discussions beyond Southeastern Europe, which has received a disproportionate amount of attention. In general, these essays (except perhaps Denis Smyth's on Spain) show a contrast between a coherent German strategy and varying degrees of British unwillingness to copy German tactics, a contrast that Recker develops in her introduction.

A Finnish historian, Merja-Liisa Hinkkanen-Lievonen, deals with the Anglo-German commercial rivalry in the Baltic states during the whole of the interwar period. During the 1920s the British showed very little interest in the Baltic states, although some in Britain initially hoped to use them to reestablish trade with Russia. The Baltic states initially resisted German efforts to build up a strong commercial connection, partly, it seems, because they feared the influence of their German minorities. Things changed after the depression when the British government concluded bilateral trading agreements with all three Baltic states and the Germans rebuilt their positions by means of clearing agreements. In general, the Germans remained the leading exporters to the region and the British the leading importers, but any political effects of those relationships were far overshadowed in 1939 by the Nazi-Soviet Pact.

Recker's summary of British and German policies toward Southeastern Europe in the interwar period is more provocative and includes a remarkable amount of analysis and summary data in only forty-eight pages. In Britain she notes three separate strands of policy toward the southeast that originated near the end of World War I: one, strong in the Foreign Office, that sought to build up an anti-German barrier of new, cooperating successor states; a second, represented by Lloyd George's advisers, that looked rather favorably on German influence in the region, partly as a check to bolshevism; and a third, strong in the Treasury and

Bank of England, that renounced sweeping political goals and favored loans to the successor states only when such loans would encourage sound finance and a growth of British exports. The third view generally prevailed in the 1920s, and the British resented French attempts to acquire exclusive economic or political influence among the successor states. The German government, meanwhile, tried to encourage economic connections with the new states but held back during the 1920s from any economic or political initiatives that might seem too provocative, either to the Western powers or to any of the new states themselves. Recker's discussion of the impact of the depression and the Nazi regime, under which German trade with the region increased rapidly and with at least the tacit encouragement of many British officials, generally accords with earlier treatments, including those of Hans-Jürgen Schröder, Berndt-Jürgen Wendt, Gustav Schmidt, and myself.

Patrick Salmon examines the Anglo-German rivalry in Scandinavia—an area that was of greater economic significance than the southeast but that has received a good deal less attention. Here he finds a similar story of British lethargy, within both industry and the government, and German enterprise, which developed more extensive trade during the 1930s within the framework of overall Nazi economic policy. Attempts to reorient German trade toward the periphery of Europe, however, could not replace German participation in the world economy. In the long run, they simply helped prepare for a war designed to put Europe's entire economic resources at Germany's disposal.

Political and economic paradoxes are considerably more apparent in Denis Smyth's discussion of German and English economic policies toward Francisco Franco's Spain. On the one hand, Smyth argues, British neutrality and patience paid off after Franco eventually won the war and returned, fairly rapidly, to the status of an economic client of Britain, just as some observers had predicted he would have to do. On the other hand, German attempts to secure control of Spanish mining interests in exchange for military support generally failed and, in Smyth's opinion, left a legacy of distrust that inclined Franco in late 1940 to refuse to enter the war except in exchange for firm promises of territorial gains.

Although all of these studies provide useful information, it may be that investigations of specific rivalries in peripheral areas cannot add a great deal more to our understanding of the influence of international trade and finance on the origins of World War II. Although trade with the European periphery was the focus of a great deal of rivalry and change, it never rivaled trade with the Americas or even among the Western European powers themselves in overall significance. Perhaps it is time for a summary study of Nazi Germany's foreign trade in particular, of the ways it contributed to German rearmament, and of the ways it helped the Germans fight World War II.

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WILLIAM I. SHORROCK. *From Ally to Enemy: The Enigma of Fascist Italy in French Diplomacy, 1920–1940*. Kent, Ohio: Kent State University Press. 1988. Pp. xii, 355. \$35.00.

This well-written, though occasionally one-sided, analysis of French diplomacy with Fascist Italy between the two world wars is based on documents opened up in 1982 by the French Ministry of Foreign Affairs and on volumes in the *Documents diplomatiques français, 1932–1939*. William I. Shorrock observes that, apart from isolated chapters in Jean-Baptiste Duroselle's *La Décadence, 1932–1939* (1979), relatively little work has been done on France's relations with Italy in these years. The author telescopes his treatment of the 1920s into three chapters, because he has previously dealt with that decade in a number of articles. Eleven chapters cover the 1930s.

Shorrock, who generally makes a positive judgment of French diplomacy, perceives three reasonably coherent periods in France's relations with Fascist Italy. The first extended from the end of the Great War to the collapse of the Weimar Republic. During the early 1920s, the French, nervous and defensive about the burdens thrust on them by the peace settlement, were inclined to take a conciliatory view of the nascent Fascist movement and regime led by Benito Mussolini.

The second phase covered the years from 1932 to the end of 1935. Hitler's rise to power brought the common German danger back into focus and initiated the most fruitful period of Franco-Italian reconciliation. This period culminated in the Laval-Mussolini accords of 1935 and the subsequent Badoglio-Gamelin military understandings. The Stresa Front declarations of Pierre Laval, Ramsay MacDonald, and Mussolini, undertaken in the wake of unilateral German rearmament, revived the hopes of those who believed (correctly, in Shorrock's judgment) that only a renewal of the World War I coalition could prevent further treaty violations by Germany.

In the final phase (1936–40), everything that had been achieved in the way of Franco-Italian reconciliation by French statesmen and diplomats, from Camille Barrère and Raymond Poincaré through Jean-Louis Barthou, Laval, and Pierre Etienne Flandin, collapsed. This period witnessed the gradual alliance of Fascist Italy and Nazi Germany and ended with the brief war of June 1940.

What led to the breakdown in Franco-Italian relations, Shorrock asks? Partly it was the Ethiopian War and the Spanish Civil War, but chiefly it was because good Franco-Italian relations were predicated on mutual cooperation between Paris and London. The Locarno and Stresa agreements are cases in point. Unfortunately, the 1930s produced few occasions of cooperation between France and Britain. For this, London must bear much of the responsibility, Shorrock argues, although he concedes that French leadership cannot be entirely exonerated. Laval, for example, was inclined to take ultimate English support for granted and thus to direct his energies elsewhere.

The most pivotal factor, according to Shorrock, was the behavior of the French Popular Front governments. Their "fumbling and dilatory diplomacy cemented the Italo-German relationship that [their] predecessors had labored so energetically to forestall" (p. 292). Their "unrelieved ideological hostility to fascism and its tendency to regard the Italian government with contempt, although intellectually honest, blinded [them] to strategic and political reality" (p. 293).

The author is more charitable toward Laval than was Duroselle. Shorrock gives high marks to most of Laval's foreign policy in the mid-1930s but does not deny that Laval erred in continuing to believe in 1939–40 that a French alliance with Italy was still possible.

One wishes that Shorrock had given attention to Italian policy making toward France and that he had used the substantial Italian documentation on this period. (He has confined his citations to Italian works that have appeared in translation.) One also regrets the lack of discussion of the intrigues of the Italian anti-Fascist émigrés in France in the 1920s and 1930s, activities that greatly annoyed Mussolini.

CHARLES F. DELZELL
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ANTHONY READ and DAVID FISHER. *The Deadly Embrace: Hitler, Stalin, and the Nazi-Soviet Pact, 1939–1941*. New York: W. W. Norton. 1988. Pp. xxi, 687. \$25.00.

Anthony Read and David Fisher, producer and writer respectively with the British Broadcasting Corporation, skillfully correlate three separate but roughly concurrent sets of secret negotiations that culminated in the German-Soviet Nonaggression Pact and the outbreak of World War II. Where most accounts concentrate on the abortive Soviet negotiations with Britain and France for a political and military alliance and the parallel Soviet-German negotiations, Read and Fisher interweave an often-overlooked third set, the British and German talks. This approach shows that, right up to September 3, 1939, Neville Chamberlain preferred further appeasement of Hitler to a grand alliance with the USSR. Chamberlain's emissaries offered Adolf Hitler "the biggest bribe in history" (p. 113). It included an enormous British loan to Germany and settlement of colonial problems and of the Polish question on terms favorable to Germany. Still more, the British offered Germany an Anglo-German condominium over Europe and all of this in exchange for a mere promise from Hitler that he would not invade Poland. But Hitler, for the moment, preferred the destruction of Poland in a short, local war to another and greater Munich agreement.

Until August 20, Stalin sought an airtight political and military alliance with Britain and France. Chamberlain and Lord Halifax in turn thought they could temporize endlessly in dealing with the USSR, blindly assuming the impossibility of rapprochement between deadly ideological foes such as Stalin and Hitler. But

Joseph Stalin (like Chamberlain and Hitler) kept a second option open. In negotiating the pact with Hitler, Stalin coolly raised his price for Soviet neutrality and succeeded in wringing every last bit of advantage out of a distraught Hitler engaged in countdown for war against Poland.

Thus, Read and Fisher label as myth the notion that Hitler duped Stalin. Stalin won from Hitler everything that Britain and France could not or would not give: peace while European capitalism fought ruinous war, recovery of lost territory, a sphere of interest in south-eastern Europe, and curbs on Japanese aggression. But Read and Fisher attach decisive importance to a Soviet gain that historians generally overlook: strategic trade with Germany. The Soviet-German trade talks and agreements were more than a mere preliminary, or *lagniappe*, to the pact. Because economically straitened Germany desperately needed Soviet-supplied raw materials, Soviet negotiators were able to extract German agreement to delivery of models of Germany's most up-to-date weaponry and vast quantities of strategic goods. Thus, by September 1939, the two leaders found themselves in "the ludicrous situation where Hitler needed food and raw materials from the Soviet Union in order to attack her, while Stalin needed machinery, arms and equipment from Germany in order to be able to fight her off" (p. 433). Hence, a "deadly embrace."

Conscious of Hitler's ultimate intentions against the USSR, Stalin, now the arch appeaser, desperately hoped until the end to gain additional time to rebuild the Red Army by buying Hitler off with fresh deals on trade. This hope, plus Hitler's skillful deception covering Operation Barbarossa, induced Stalin to dismiss reports of imminent attack by Germany, especially as dates in May reported for the invasion passed peacefully.

Without exculpating Stalin of his numberless crimes, Read and Fisher depict quite favorably his dealings with Hitler. Paradoxically, this book comes just as the Soviet historians' reassessment of Stalin's German policy is moving to a highly negative evaluation.

ALBERT RESIS
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JOHN JONES. *Balliol College: A History, 1263-1939*. New York: Oxford University Press. 1988. \$75.00.

John Jones has written a lively, affectionate, and considered history of Balliol college. Although he does not find any persisting themes, his narrative reveals that the influences that acted on the college and the character of its students and fellows were determined in varying proportions by family, region, religion, and politics. Until the late nineteenth century, Balliol was essentially an autonomous community troubled occasionally by the often-ineffectual intrusion of the outside world. The most constant threats to Balliol's existence

came from financial crises and the virulence of religious and political issues.

John de Balliol, a northern baron, founded a community of scholars in Oxford in the 1260s, and his formidable widow, Dervoguilla of Galloway, created a college with its own statutes. In the fifteenth century, the small community was reorganized by Bishop Richard Fox's statutes as a medieval monastic institution, tightly disciplined and devoted to logic, philosophy, and theology with canon law left as a long vacation pursuit. Fox's statutes of 1507 survived intrinsically for the next three and one-half centuries. Although little is known of internal matters, Jones finds signs of a strained atmosphere during the Reformation that reflected national uncertainties. Elizabethan Balliol participated in the increased demand for university education, and, from the early seventeenth century, the college's finances were strengthened by bequests. Teaching was concerned with rhetoric, Greek language, and logic with occasional lessons in divinity. The college was relatively small, but it had good connections. Balliol men consistently entered important offices in church and government, and, as a result of increasingly Puritan inclinations in the early seventeenth century, some became pioneers in America. In Oxford itself, the college became prominent only in the 1830s with the introduction of open scholarships and fellowships.

Balliol took a leading role in university reform with the creation of the Royal Commission in 1850, which included Balliol men, and especially through the efforts of Benjamin Jowett, who labored for a generation before his election as master in 1870 until his death in 1893. Through the nineteenth century and well into the twentieth, no other college could compete with Balliol in producing graduates who collected the greatest number of distinctions in college and in the world. Jones argues that students took seriously Jowett's injunction to work hard, using their opportunities and talents to overcome difficulty. But he also accepts Hastings Rashdall's explanation that, because Balliol's scholarships were opened to general competition, it enjoyed the pick of the secondary schools. Although both reasons are plausible, it might be added that what undergraduates were taught, especially in the two most popular disciplines of history and the classics, led them to enter public service as a duty. A chapter on World War I reveals the college's losses and its impetus for reconstruction under A. L. Smith, elected master in 1916. A brief discussion of Balliol between the wars concludes somewhat inconclusively in June 1939, when the Balliol men in residence were exiled to Trinity. Jones believes that an objective appraisal of the last fifty years will have to wait another fifty years.

This book should be read by anyone interested in the development of the British university. Begun as an archivist's account of parochial matters, the study reveals an intimacy with college records and a perceptive

analysis of their meaning within the changing circumstances of seven hundred years of university life.

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ROGER B. MANNING. *Village Revolts: Social Protest and Popular Disturbances in England, 1509–1640*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xiii, 354. \$74.00.

In this book Roger B. Manning shows that rural protests of the sixteenth and early seventeenth centuries were usually a response to changes in land use (particularly enclosure of commons and wastes) that threatened common rights or customary tenures and obligations. Drawing extensively from secondary works as well as from his own research into central court records, especially those of Star Chamber, Manning presents detailed case studies of several kinds of village revolts—anti-enclosure riots, tenurial and rent protests, and poaching—and he shows how demographic expansion, agrarian change, and the growth of rural industry contributed to such disturbances. For purposes of comparison, he also includes a chapter on apprentices' riots and other popular protests in London.

Manning argues that such revolts were characterized by localism: they were concerned with particular grievances and directed against particular persons, as well as being small in scale and confined to one or a few villages. A primitive and prepolitical form of protest, they seldom posed a threat to the existing social order. During the sixteenth century, enclosure riots and other village revolts were often procured or led by gentlemen, and they displayed few signs of class conflict. Although Manning suggests that it was only in London, and during the reign of Charles I, that popular disturbances displayed a fully developed political awareness, he does point to some changes in the character of revolts during the late Elizabethan and early Jacobean periods. Not only did village protests become more numerous, larger in scale, and more elaborate, but those years also witnessed two regional rebellions against enclosure, the abortive Enslow Hill Rising of 1596 and the Midland Revolt of 1607, as well as an "Epidemic of Disorder" (pp. 200ff.) in London. Smallholders, artisan-cottagers, and laborers were prominent in the disturbances, which displayed a greater degree of social conflict than earlier protests and even a few hints of political consciousness. Manning attributes the changes to the economic, social, and political crisis of the 1590s and to the fact that the revolts came to be concentrated in forest and pastoral areas where there was growing social cleavage between the propertied and the landless.

This book is a major addition to the literature dealing with social protest in early modern England, offering a richly documented portrayal of the diversity

and complexity of sixteenth- and early seventeenth-century popular disturbances. Manning rejects a single model of agrarian change, and he also eschews a single model of early modern riots. Although he takes into account the work of historians, such as Buchanan Sharp, Keith Lindley, John Walter, and Clive Holmes, his evidence suggests that village revolts of the Tudor and early Stuart periods were too diverse in motivation and character to fit any one typology.

In his expert discussion of the government's response to riots and of the circumstances in which the courts came to distinguish between private disputes and public protests, Manning also contributes to our understanding of the development of the law of public order. At the same time, he shows that the protesters' awareness of the penalties for riot and an understanding of their legal rights influenced their actions, and he provides further evidence of the use of the courts by seventeenth-century English villagers.

Manning's book, with its emphasis on community and localism, also contributes to ongoing debates about the nature of English society and culture in the early modern period. Manning challenges Alan Macfarlane's claims about the extent of individualism in English society, arguing that a sense of community is evident in villagers' attempts to defend common rights against claims of private property. The book, however, also contains evidence of divisions within villages and manors, and "community" was sometimes redefined to include only the propertied. Although Manning's emphasis on the localist character of agrarian revolts is largely convincing, there remains the question of whether such protests provide an accurate guide to the level of political consciousness in seventeenth-century English villages. Did villagers' defense of custom and tradition sometimes display a more political dimension than is revealed in agrarian disturbances (for example, in the case of protests against the fiscal and military demands of the state)? While historians continue to debate those larger questions, this book deserves to be widely read for its detailed and thoroughly documented portrayal of sixteenth- and seventeenth-century agrarian protest.

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SERGIO AIOLFI. *Calicos und gedrucktes Zeug: Die Entwicklung der englischen Textilveredelung und der Tuchhandel der East India Company 1650–1750*. (Beiträge zur Kolonial- und Überseegeschichte, number 40.) Stuttgart: Steiner, 1987. Pp. 441. DM 76.

In the seventeenth century, the English took to cotton cloth as a cheap substitute for linen. Like linen, cotton could be used to make underwear and nightshirts, tablecloths and bed coverings. It could even be used for bagging. Cotton gloves and handkerchiefs were also cheap substitutes for silk ones, while chintz competed with English woolen goods as a decorating material and

in some forms of clothing. Between 1620 and 1690 cotton imports jumped from 120,000 to 700,000 pieces.

Legislation in 1701 prohibited the importation of dyed, but not of white, Indian cloths. This action favored the development of English print works, which had been established in the late 1670s. Copper plates were used for printing until the development of a rotary printing process in 1783. Hand painting by European workmen would have been too expensive, although the hand-painted Indian cloths were the most fashionable even after the prohibition of 1701.

English print and dye works were concentrated around London, near the source of the cloth. They were substantial affairs, employing two or three hundred workers apiece, but they were not good neighbors. Many fires produced much smoke, while the bleaching process required large bleaching fields before the introduction of sulfuric acid. The operations also required unusual quantities of water.

In India, craftsmen did rather well when two European powers competed for a product, as before 1688 and after 1720. But, for Indians, weaving was a seasonal task, as bleaching was for the Indian bleachers, and their seasons did not fit too well with the best times for setting sail from Madras. They did not like to weave cloths of the size that the Europeans wanted to buy, their work was not uniform, and they demanded advances for work that was not always delivered. By the middle of the eighteenth century, the English were beginning to find it more convenient to have their weaving done at home and thus began one aspect of the Industrial Revolution.

Sergio Aiolfi's dissertation is useful, and the photocopied typescript is not hard to read. But it is a dissertation, not a book. I question one point: he suspects that the East India Company was trying to export English weavers to India by calling them soldiers, hoping perhaps to use them in industrial espionage. Yet studies of the armies sent to fight in America in the 1770s indicate that the typical soldier was a twenty-nine-year-old failed weaver. It seems likely that there were many unemployed weavers joining the forces rather than that there was any conspiracy.

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LOIS G. SCHWOERER. *Lady Rachel Russell: "One of the Best of Women."* Baltimore: Johns Hopkins University Press. 1988. Pp. xxviii, 321. \$29.50.

Lois G. Schwoerer's biography of Lady Rachel Russell is a lucid, valuable account that augments and vivifies our knowledge of aristocratic existence in late seventeenth-century England, adding significantly to the burgeoning recovery of early modern women's experiences and attitudes. Unlike many women of her class and era, Lady Russell, the wife of Whig "martyr" Lord

William Russell (executed in 1683), has been the subject of previous biographies, and her letters have been published. But, as Schwoerer makes clear, these largely nineteenth-century efforts, aimed at preserving Lord Russell's reputation rather than exploring Lady Russell's life, portrayed her "as a mourning wife and devoted mother" (p. xxiii). In contrast, Schwoerer seeks to show how a traditional, aristocratic woman "exploited the opportunities available to her to escape conventional restraints without condemning them" (p. xxvii).

Schwoerer demonstrates that Rachel Russell's long life (1637–1723) was dominated by four consistent themes: politics, piety, property, and passion. Daughter of the French Huguenot Rachel de Ruvigny de la Maison Fort and Thomas Wriothesley, the fourth earl of Southampton, Rachel grew up in an Anglican household "deeply marked by respect for Puritanism" (p. 19). Married to Lord Francis Vaughan in 1654, she found herself a childless widow at thirty. Slightly over two years later, in 1669, she married the handsome, impulsive, chivalrous, extravagant William Russell (second son of the fifth earl of Bedford, who eventually inherited the title), and, as Schwoerer demonstrates from the ample surviving correspondence, the marriage was a passionate, fourteen-year love match. "If I love once, I shall do so ever," Rachel had declared (p. 33), and she did. Although neither William nor Rachel was an intellectual, they were both committed to a vehemently anti-Catholic politics of dissent, and Russell eventually became the leader of the Whigs in the House of Commons. After her husband's execution for treason, Rachel upheld these principles throughout her long widowhood in which, as a Whig matriarch, she exercised influence in the patronage systems of both church and state. Maintaining extensive connections with her own blood relatives and with relations acquired through both her marriages, Lady Russell was also extremely skilled at managing property, consolidating wealth, and arranging marriages, much in the manner of a competent and influential patriarch.

The most exciting section of the book concentrates on Lord Russell's trial, a fact that also constitutes the one problem with this excellent biography. Schwoerer's reassessment of the trial comprises an elegantly written chapter in early modern political and legal history, and the careful, lucid examination of the laws governing seventeenth-century treason trials reveals the essential fairness of Russell's trial and the subtle and self-interested ways in which his martyrdom was constructed. So compelling does this account become that it dwarfs Schwoerer's more dutiful rendition of Rachel's complex business and property transactions during her widowhood; the value and significance of the forty-year widowhood become an epilogue to the central drama of the fourteen-year marriage, trial, and execution. Although Schwoerer reiterates that Rachel had long "established a reputation for intelligence, courage, good sense, piety, and loyalty" (p. 189), we continue, as this quotation suggests, to view Rachel

from the outside. Despite the fact, for example, that many of Rachel's writings survive, including letters, essays, and two efforts at autobiography, none of these is ever granted centrality or examined systematically. Although Schwoerer recognizes what she describes so well as "the public dimension of family culture" (p. 71), she privileges political over private life. Although she acknowledges some of the contradictions of being a powerful woman in Restoration England (for example, Rachel's identification with men and the view of her as an "exception" among women), she never explores or vivifies them.

As Schwoerer herself realizes and demonstrates amply, one of the major revelations of Rachel Russell's life is its problematic refutation of assumptions about female powerlessness in the seventeenth century: Lady Russell's reach and her influence over politics, her children's education and marriages, and the transmission of property were enormous. The careful, substantial research as well as the eloquence and clarity of this biography make it an important contribution to women's history and to all scholarship concerned with early modern English life.

MARY BETH ROSE
Newberry Library

JOHN B. HATTENDORF. *England in the War of the Spanish Succession: A Study of the English View and Conduct of Grand Strategy, 1702-1712*. (Modern European History.) New York: Garland. 1987. Pp. xxii, 408. \$60.00.

This study of Queen Anne's War is a revised dissertation by a former naval officer who teaches maritime history at the Naval War College at Newport. Its title recalls Sir Julian Corbett's classic work on the Seven Years' War, but its subtitle indicates its more limited aim, which is to define England's strategy in the war and to comment on how, and with what success, that strategy was carried out.

England's central war aim was at bottom defensive: to safeguard the Protestant Succession after the death of Anne by reducing, or at least containing, the power of France, should the French be tempted in the future to intervene on behalf of the Stuart pretender. Because the English, even together with their Dutch allies, lacked the land forces required to teach Louis XIV the necessary lesson, it was essential to build a broader network of offensive alliances with continental powers opposed for a variety of reasons to a French succession to the vacant Spanish throne; the Austrian emperor was, of course, chief among these, for dynastic reasons. To support and encourage these allies, England's main efforts had to be made against the French forces in Flanders, Bavaria, and Spain. The English did mount campaigns in Canada and the Mediterranean, but they were mainly diversionary, designed to draw French strength from the principal theater of war. (Interestingly this strategy was the exact opposite of William Pitt's fifty years later.)

John B. Hattendorf's book is most successful at this general level of discussion but much less so on the underpinnings of fact and actual decision making by individuals. "No single document has been found which adequately expresses England's strategy," he writes (p. 64), nor did any "single individual express [it] fully." It must therefore be discovered by a "process of deduction" and "constructed artificially" (p. 54).

Who, then, was in charge of it all? In Sir George Clark's *Later Stuarts, 1660-1714*, this was "Marlborough's War," but here the great duke is little more than an executor of bureaucratic directives. Hattendorf makes a half-hearted attempt to assign a key command role to Queen Anne herself and her dull consort, Prince George (p. 335, n. 54), but his suggestion is not convincing. In the end he attributes the conception and direction of the war effort to "the collective efforts of [unnamed] English officials" (p. 51) and resorts frequently and irritatingly to personification, as in "England believed" (p. 115) or "England realized" (p. 265). In similar bureaucratic fashion, he talks of "the attitude of Parliament," as if that body were not sharply divided between Whigs and Tories with widely differing ideas on almost every key issue. Indeed, Hattendorf is not well informed on the politics of the period, which in any case he dismisses as mere "party squabbles" (p. 209) and "Whig-Tory ballyhoo" (p. xiii).

A strength of the book is the author's wide reading, especially in archival sources both in England and abroad that bear on diplomacy. In sum, specialists in military aspects of the reign of Anne will find here things both to note and to dispute. But historians of the broader eighteenth century will still await a new Corbett.

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CHARLES H. FEINSTEIN and SIDNEY POLLARD, editors. *Studies in Capital Formation in the United Kingdom, 1750-1920*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xvi, 477. \$96.00.

Debate over the sources of economic growth in the 1960s initiated a series of country studies whose purpose was to develop a historical data base adequate to accommodate serious empirical analysis. Because capital accumulation and investment behavior were central to the debate, scholarly attention quite naturally turned to such matters early on. Charles H. Feinstein and Sidney Pollard have led this academic enterprise in Britain, and all of us who have been working on the first Industrial Revolution from its start in 1750 to the productivity slowdown in the late nineteenth century have had frequent occasion to rely on their efforts. This volume is the ultimate achievement of a long series of publications, representing a careful revision of earlier tentative estimates.

The book is in two parts. The first, based largely on archival and other primary sources, contains nine sec-

torial studies authored by John Butt, Stanley Chapman, R. S. W. Davies, John Ginarlis, B. A. Holderness, D. T. Jenkins, John Orbell, Jennifer Tann, and, of course, Sidney Pollard himself. The studies cover the century from 1750 to 1850 and include agriculture, coal mining, the iron, cotton, wool, and corn milling industries, steam power, roads, and waterways. None of these studies attempts analysis; rather, the essays dwell on the construction of capital formation estimates. In the second part, Charles Feinstein presents a revision of his earlier, and oft-cited, capital formation and capital stock series. They displace the estimates in two previous publications, for 1760–1860 (Feinstein, *National Income, Expenditure, and Output of the United Kingdom, 1855–1965* [1972]) and for 1856–1920 (Feinstein, “Capital Formation in Great Britain,” in P. Mathias and M. M. Postan, eds., *Cambridge Economic History of Europe*, 8 [1978]), so that for the first time we have a comprehensive set of national estimates compiled on a consistent basis for the entire period from 1760 to 1920.

The book represents an extraordinary achievement. The estimates are derived with unbelievable care, and one sets the book down in the belief that there are simply no scraps of archival information that have been left untouched and that there are no economic historians who know better how to use them.

Having praised the volume, I must now report its flaws. There are many, and the critique that follows dwells on part 2 because Feinstein's estimates will become the key reference for any scholar interested in British investment and accumulation from the mid-eighteenth century to World War I. In the first place, the contributors to the two parts of the book make no effort whatsoever to communicate with each other. Has Feinstein used the sectorial studies in part 1? He almost never cites them. Does that imply a judgment on their quality? In the second place, I was unhappy with Feinstein's disarming discussion about the reliability of the data (pp. 264–65). Although good scholars applaud this kind of caution, potential users will not find very helpful a statement that the margins of error are likely to be around 20 to 25 percent in most of the components of the series (with no mention of the direction of error). With errors that large, should we not suspend all serious quantitative analysis of British historical experience with accumulation? The answer, of course, is no. It depends on how the data are used. As guardian of these data, Feinstein has a responsibility to tell us where the estimates are weakest, for what uses they will be least effective, and on what assumptions the estimates are most sensitive. Nowhere in part 2 does Feinstein accept that responsibility. In the third place, Feinstein makes no effort to compare his new estimates with his earlier ones. Indeed, there is no discussion in part 2 at all on the new estimates for the period from 1760 to 1860, which encourages the mistaken belief that the revisions of the first century of the time series are trivial and cosmetic. Not so. The capital formation estimates for the 1760s and 1770s have been raised

from 15 to 40 percent, while they have been lowered from 8 to 12 percent for the period between the 1820s and the 1850s. As a result, whatever evidence there was of rising investment shares in gross national product between 1760 and 1860 is now pretty much erased. Finally, none of the authors in this volume makes any effort whatsoever to confront historical debate with these new estimates. No doubt the rest of us will, but, had Feinstein and Pollard done so, we might have emerged with a better appreciation of the strengths and weaknesses of a remarkable set of data.

Although this volume has big flaws, historians of the British economy must go out and buy the book. It will remain a key reference for many years to come.

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JESSIE PARFIT. *The Health of a City: Oxford, 1770–1974*. Oxford: Amate. 1987. Pp. xii, 152. Cloth \$22.00, paper \$15.00.

This book is a short administrative history of public health in an important, if atypical, provincial English town. Jessie Parfit's prime interest is the rise and fall of the local health department and its physician director, the medical officer of health.

Municipal health services developed quickly in Oxford after the appointment of the town's first full-time medical officer in 1902. In Oxford, as in the rest of the nation, local authority health departments achieved their peak influence between the creation of the Ministry of Health in 1919 and the beginnings of the National Health Service in 1948. The medical officer of health of the more active local authorities administered in these years a comprehensive public health program that was a product of initiatives in basic sanitation begun in the nineteenth century, personal medical services instituted in the early twentieth century, especially during the war, and institutional and home services to the blind and the handicapped transferred to local authorities when the Poor Law Guardians were abolished in 1929. Health departments then employed physicians, dentists, midwives, and nurses. They not only sought to control environmental hazards but also diagnosed, isolated, and treated cases. They ran outpatient clinics, hospitals and other residential institutions; they supervised the school medical service; and they provided care and support for the handicapped and the chronically ill.

Subsequent policy changes fundamentally altered and then emasculated this local medical system. The National Health Service Act (1948) transferred local authority hospitals and treatment centers to the newly created regional hospital boards. Legislation between 1969 and 1974 dismantled most of what remained, transferring services to national therapeutic and social service agencies and abolishing the position of medical officer of health.

Parfit, a former public health official in the 1950s

and 1960s and the spouse of the former medical officer of health for North Berkshire, has had much experience in public health work under local authorities and feels strongly that something important has been lost with its demise. Her account offers a quite thorough, if brief, summary of administrative arrangements and public health services in Oxford. But, apart from occasional comparisons to events in Reading, Edinburgh, and Glasgow, the book offers little historical interpretation. The absence of a strong analysis is disappointing because recent monographs—for example, Anthony S. Wohl's *Endangered Lives: Public Health in Victorian Britain* (1983), J. M. Winter's *The Great War and the British People* (1986), and Jane Lewis's *The Politics of Motherhood: Child and Maternal Welfare in England 1900–1939* (1980)—raise important historical questions that a specialized study of public health in one local authority may help answer. Even in dealing with the demise of local health departments, Parfit does little to help us understand why the changes she chronicles and regrets occurred. She suggests that administrative changes were related to the growing popularity of social medicine, but she does little to probe its meaning, to explain its growing popularity, or to identify its historical roots. Throughout her account, we are given little insight into motives, ideas, and alternatives. One wonders, in fact, how much light an administrative history can throw on the problem of why a local medical system that served so well in the 1920s and 1930s was found to be inadequate in the postwar era. Parfit mentions that, in Oxford, general practitioners had begun to dominate infant and maternal care in the 1960s and that in the same years there was a trend away from home-based midwifery services. One suspects that the administrative changes that are the focus of this book are best seen as reflections of changes in the attitudes of patients and their physicians toward local government. Such changes in attitude are not explored in this book.

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DONALD READ. *Peel and the Victorians*. New York: Basil Blackwell. 1987. Pp. xi, 330. \$49.95.

This book should be titled "Peel and Public Opinion," for Donald Read's purpose is to examine the relationship between Robert Peel and the opinion makers of his day. This is, however, a curious topic for a lengthy treatment. Although Peel was obviously aware of the importance of public opinion, rarely did he court it. His infrequent speeches outside Parliament were usually given to selected audiences. His famous appeal to a national electorate, the "Tamworth Manifesto" of 1834, was an experiment never repeated. Apart from feeding a few exclusive stories to the *Times*, he never tried to control the newspaper press as did his contemporaries Henry Brougham and Lord Palmerston. In-

deed, as Read himself observes, Peel "was uneasy" about the "pretensions" of the newspaper press "to represent and influence public opinion" (p. 315).

If not a creator or manipulator of public opinion, was Peel nevertheless responsive to it in framing his legislative policies? We know that he was an inveterate newspaper reader and kept himself well informed as a matter of ministerial duty. But there is little convincing indication in Read's account that public opinion influenced Peel's legislative program. Even his change of mind over the Corn Laws in 1845 can be attributed less to the arguments of the Anti-Corn Law League than to his concern about the Irish famine and the threat to order it posed. Decisions of state should be made, Peel firmly believed, without reference to the crosscurrents of party and the contentiousness of interest groups. Good government was a matter of executive action. Peel's inherent shyness and reserve confirmed his reluctance to involve himself in the hurly-burly of Victorian politics, whether inside Parliament or "out of doors."

Given the lack of evidence for an active interplay between Peel and public opinion, Read is often forced into the familiar story of Peel's official life. This approach is serviceable enough, and the book is well-researched. But it is eulogistic, and the interpretation is rather old-fashioned. Read finds Peel "impeccable" in private life (p. 18), "masterly in argument" (p. 34), and growing in "magnetism" (p. 158) during his great ministry of 1841–46. Read lacks the subtle observations of Norman Gash, whose work on Peel remains authoritative.

The fundamental problem of Read's book is that it does not define public opinion, nor does it analyze its constituent parts. Read does not mention the role of public petitioning, an instrument much used by the Chartists and the agricultural protectionists for their very different purposes. He ignores county meetings and other forms of public gatherings, which were significant to the Irish Repeal movement of the 1840s. There is no adequate analysis of the newspaper press as an engine of public opinion: Read's discussion is largely limited to quotations from the leader columns. The work of Jeremy Black and Stephen Koss, who have much of value to say on the role of the press in politics and society, would have been useful here.

The most promising part of Read's book is his penultimate chapter, "The Victorians and Peel," in which he discusses Peel's growing popularity especially after his unexpected death in 1850. As Read rightly observes, Peel was not only the object of a genuine outpouring of public sentiment but also a focus for enterprising memorializers. Busts, engravings, Staffordshire figurines, medals, and cartoons bearing Peel's likeness all sold well. Developed further, this chapter would be a useful short study on Victorian image making and its commercial purveyors.

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HELMUT REIFELD. *Zwischen Empire und Parlament: Zur Gedankenbildung und Politik Lord Roseberys (1880–1905)*. (Publications of the German Historical Institute, London, number 18.) Göttingen: Vandenhoeck und Ruprecht. 1987. Pp. 228. DM 64.

This book springs from Helmut Reifeld's belief that the many historians who have interpreted Lord Rosebery's career in terms of his undoubted political ineffectiveness have failed to understand that Rosebery's historical significance is to be found in his thought rather than in his action. Rosebery is here presented as a man of significant insight into the tensions between the requirements of empire and the nature of democratic party politics. We are told that by 1896 at the latest he had abandoned his earlier, more sanguine beliefs and had recognized, as hardly any other leading politician of his time had done, that the maintenance of the British empire was fundamentally incompatible with the continued process of democratization at home.

It is hard to believe that the banalities that are here drawn from his speeches and writings are really all that Rosebery had to say. It may well be that, in his determination to separate Rosebery's principles from any comments that had an immediate political relevance, Reifeld has left us only the commonplaces, useful enough in the context of a speech designed to achieve a specific effect but of no great moment otherwise. Although what strikes British natives as mere commonplaces could conceivably have gained a deeper significance in the eye of an outsider, this does not happen here in any convincing way.

After a methodological introduction, the book embarks on an exposition of Rosebery's thought. Chapter 2 on liberalism and imperialism and chapter 3 on reform politics are followed by a long chapter on Rosebery's involvement with various issues of foreign policy. This material is then pulled together in chapter 5, where three constant factors in Rosebery's thought are presented—his concern with a divided liberalism, his criticism of party democracy, and his belief in the importance of continuity in foreign policy for the security of the empire. This division of themes does not work well; it separates things that are better considered together. But the real problems of presentation are the result of writing too close to the text. German readers would have benefited from a more comprehensive historical exposition of what was at stake.

Reifeld is, unfortunately, too frequently at a loss himself. His reluctance to regard Rosebery as a Whig is the result of his belief that Whigs were necessarily opposed to the spirit of reform (p. 29) and identified with class politics (pp. 175–76). His assessment of the tension in Rosebery between traditional parochial loyalties and concepts of modernization is based in part on the use of organic metaphors as evidence for the parochial orientation. Reifeld is apparently unfamiliar with the impact of Darwinism on the imagery of late nineteenth-century politics. Then there is the problem of "single issue politics," a key concept for any under-

standing of the internal tensions of the Liberal party. Reifeld thinks that, when Rosebery identified himself with single-issue politics in opposition to "programme politics," he was expressing a preference for the customs of the period before 1867. If he is unaware that Gladstone had practiced single-issue politics from 1868 for two decades at least, what will his German readers make of it all?

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Y. CASSIS. *La Cité de Londres, 1870–1914*. (Modernités XIX^e–XX^e.) Paris: Belin. 1987. Pp. 235.

It is a curious fact that the commanding heights of the British economy—the institutions of finance capital—have received so much less attention from historians than the relative flatlands of manufacturing industry. We know a great deal more about the economic and political history of Lancashire and Birmingham than about the City of London, although London, even at the floodtide of the Industrial Revolution, was a much more powerful invisible presence in British public life than cotton or iron and steel. In recent years, however, there has been some redress of that neglect. The work of W. D. Rubinstein on millionaires, Sidney Chapman on merchant banking houses, and Kathleen Burk on financial diplomacy, for example, has begun to uncover the central role of British financial institutions not only in the working of the international economy but also in the domestic hierarchy of social prestige and political power.

Y. Cassis's study is a welcome addition to that new literature, not least because it draws together and synthesizes different aspects of a subject too often atomized into different areas of specialist expertise. It covers that unique period in international financial history, after the Franco-Prussian War had destroyed the credit of Paris and before World War I had torpedoed the pound, during which London enjoyed undisputed supremacy in the financial markets of the world. Cassis analyzes the changing structure, personnel, and day-to-day operations of the many different types of financial institutions of the period: private and joint-stock banks, merchant banks, colonial banks, foreign banks, the stock exchange, and insurance companies—not to mention the shadowy and often ill-defined operations of individual private financiers.

The period saw a great deal of structural change, particularly the conversion of family firms into public companies and the swallowing up of small-scale and country banks by larger metropolitan concerns. By 1909 there were only two private deposit banks left in the City, and the Bank of England itself was increasingly dependent on the voluntary cooperation of the larger commercial banks in its control of money markets. Nevertheless, Cassis emphasizes, there was much old wine in new bottles: partners in old concerns often reemerged as managers in new ones, and many old,

idiosyncratic practices survived. Merchant banks in particular, functionally often the spearhead of imperialist venture capitalism, remained structurally wedded to family inheritance and the office technology of the confidential clerk. Moreover, in spite of the global expansion of the City's business, the social structure of the City remained a *gemeinschaft*, closely knit together by common interests, functions, and family ties. The segregation of City interests from those of a wider manufacturing economy was strikingly demonstrated in a number of ways: British industrial securities quoted on the stock exchange fell from 70 percent of business in 1853 to 9 percent in 1913; City daughters often married into the landed aristocracy, sometimes into mass retail, but never into the manufacturing class. A banker-cum-manufacturer such as Dudley Docker, who canvassed the idea of a German-style industrial bank, stood out as the exception that proved the rule. Cassis cites a comment of 1911 that "Londres est souvent plus concernée par ce qui se passe au Mexique que dans les Midlands et plus troublée par une grève dans un chemin de fer canadien que dans une mine de charbon du Pays de Galles" (p. 48).

This book is not definitive, as it could scarcely be in the short space of two hundred pages, and it leaves some important questions unanswered (such as the unexplored paradox of City politics in the Edwardian era: ultra-Conservative but also ultra-free trade). Nevertheless, it packs a vast amount of fascinating information into a very short span, is written with great clarity, and opens up an immensely important subject. It is very much to be hoped that such a seminal work will appear in an English edition before very long.

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ANNE SUMMERS. *Angels and Citizens: British Women as Military Nurses, 1854-1914*. New York: Routledge and Kegan Paul. 1988. Pp. xii, 371. \$35.00.

This study is the first modern history of British women as military nurses, but it is much more than that. Anne Summers has placed the very small group of career army nurses in a larger context, highlighting the ways in which changing public attitudes toward these nurses were part of the progressive militarization of society. This is an important book far beyond its immediate subject matter and should be required reading for anyone interested in the relationship between noncombatants and war, between women and the military, and between social classes and job opportunities.

Historians of women will find a cogent analysis of the changing attitudes of and about women who entered nursing. Traditionally nurses were from the same class as domestic servants. Florence Nightingale, along with many other reformers, thought nursing should be

made attractive to the "better sort" of domestic servant, while upper-class ladies would be separately recruited to become matrons and head nurses (sisters). This dual-entry system proved to be extraordinarily long lasting; generations of conflicts within military nursing, as well as within nursing as a whole, can be traced to class differences among women forced to work together, to blocked aspirations for greater status, and to conflicts between lady nurses and their less-than-genteel male superiors.

Teasing information from the sources, Summers re-creates the upper-class world of the self-abnegating ladies who volunteered to serve in the Crimea, as long as they did not have to wear the hideous uniform and could be waited on by the paid nurses who accompanied them. Nearly half a century later, hundreds of ladies volunteered to go at their own expense to nurse in the Boer War. They scorned any imputations that they were not strong enough to survive under canvas and without servants; they courted publicity, as well as prospective husbands. They also made good nurses in the emergency conditions of the time, according to Summers, in spite of the resentment of the professional nurses, who felt that the ladies upstaged them for attention and medals.

In her final chapters, Summers moves away from a close analysis of military infighting to examine the connection between an increasingly militarized society and the acceptance of women nurses in war. If the army was to move successfully from being a small, separate, professional fighting force to a large, volunteer army, its leaders had to be responsive to the expectations of its citizenry, and these included better nursing. But of even greater importance, according to such reformers as Alfred Keogh and R. B. Haldane, was the training of women in order to free as many able-bodied men as possible.

Summers's detailed examination of the invasion fears of Edwardian England convincingly demonstrates the eager participation of women in bellicose patriotism. Three separate voluntary organizations trained some fifty thousand women to be nurses and leaders in case of war. Women enthusiastically took up war games that promised adventure and leadership. They and their brothers had been told for years that war was the arena in which to test one's mettle. The results, as with all voluntary efforts, were uneven. Many women learned little, and funds were never sufficient for the uniforms, tents, and other equipment considered necessary for preparedness. Nevertheless, Summers argues, "The VAD [Voluntary Aid Detachment] organisation did more than facilitate the mobilisation of women for war service in the technical sense. It also mobilised them psychically, preparing thousands to look forward to a time when their public importance would be intensified—when they would be real actors in the world 'show'" (p. 286).

I disagree with Summers on a number of details and emphases. In her eagerness to resurrect the forgotten heroines of the Crimea, she is rather unfair to Night-

ingale. The jingoism she has so eloquently documented as characterizing the period from 1890 to 1914 competed with numerous alternatives, including pacificism, international socialism, and Tolstoyism. Nevertheless, this is a thoughtful and original book that makes a major contribution to the history of the period. Summers concludes with a brief examination of the ways in which popular militarism during the Edwardian period precluded an intelligent understanding of international politics. The lessons for today are obvious.

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ROBERT H. LARSON. *The British Army and the Theory of Armored Warfare, 1918–1940*. Newark: University of Delaware Press or Associated University Presses, Toronto. 1984. Pp. 265. \$35.00.

Well into the 1930s, the British army yielded pride of place to no other when it came to experimentation with the tank, the new weapon of mechanized warfare. It was the British, after all, who had invented and first employed the tank in combat in World War I. Thereafter, for the next two decades, the theories of B. H. Liddell Hart and J. F. C. Fuller advocated the reshaping of British military policy around a doctrine of mobile warfare in which the tank was the crucial element. And finally the army itself, abandoning "dull mass" conscription after 1918, returned to what it had historically been: a long-service body of professional soldiers. In the eyes of the reformers at least, it thus offered excellent prospects for evolving into the kind of elite, highly skilled *armée de métier* supposedly best suited for the technological complexities of machine warfare.

That the early stages of World War II showed otherwise has remained an issue of fundamental importance to historical scholarship of the military experience. Those stages represent, after all, a study in military institutional effectiveness—or the lack of it—within the context of total war. To account for the British inability to cope with the Germans' blitzkrieg techniques, first in France in 1940 and, later, 1941–42, in the North African desert, historians have offered a number of interpretations: that prewar British governmental policy failed to foresee and thus failed to require the army to prepare for another major war in Europe; that lean depression-era defense budgets were inadequate for training and accession of equipment; that a basic social conservatism within the army, supposedly exercised through a dominant clique of gentry-class cavalry officers, effectively resisted change and modernization; that the inability of the army itself to sort out the theories of its reformers produced an unwieldy and unwarranted specialization of formations and roles; and, finally, that the losses of 1914–18, as well as a faith that air and sea power could adequately

secure British defense needs, led to a lack of public support for substantive ground combat preparations.

It is the contribution of this excellent analysis by Robert H. Larson to assess and put into perspective each of these possible causes. He then goes on to argue persuasively that the reasons for failure must lie elsewhere, rooted in some deeper aspect of the British approach to warfare. For, in relative terms, the British army was not at all as well prepared in 1940 to take on a first-class enemy as it had been in 1914. British flaws in 1914 were, to be sure, considerable: for example, the utter absence of plans to put manpower and industry on a total war footing. Still, though, once committed in secret prewar talks to taking up a position on the French left flank, the army gained a clearcut mission that facilitated operational competence. It thus played a role in denying the Germans, in the opening actions, the kind of *kesselschlacht* battle-of-annihilation central to Moltkean doctrine.

Not so in 1940, however. Larson shows that the British, like the Germans, also operated in concert with historical ideas governing strategy. And therein lay the problem. Whereas the Germans meticulously adapted the new, all-arms, armored-task force panzer division to the historic concept of the deep-penetration decisive battle, the British remained wedded to their historic attrition-strategy approach. The result was twofold: the British were incapable not only of waging such battles but also of recognizing that the Germans would employ in bold fashion armored forces to win the kinds of decisive battles denied them in the previous war. The British were thus doomed to lose battles that they should—based on the resources available—have won and to achieve only limited gains in victories that could have been decisive.

Well-written, rigorously researched in War Office and other official sources, this book is an essential addition to those interested in the British way of warfare—or those interested in the way that armies attempt to depart from the known past to prepare for the unknown future in the dreadful contest of warfare.

JOHN W. GORDON
The Citadel

PETER WEILER. *British Labour and the Cold War*. Stanford: Stanford University Press. 1988. Pp. ix, 431. \$42.50.

The policies of Clement Attlee's Labour governments (1945–51) and of the post-World War II Trades Union Congress (TUC) come under scrutiny in this excellent book. Most recent studies of Labour during this era (those by Kenneth Morgan, Henry Pelling, and Alan Bullock, for example) have celebrated Labour's achievements, but Peter Weiler takes a more critical tack. In his view, Attlee's governments, ably seconded by the apparatchiks of the TUC, took advantage of conditions created by the cold war to pursue traditional, conservative British aims abroad and, to a degree, at home (although he is less concerned with

those). Indeed, Weiler contends, the cold war was so useful to British Labour leaders that they, as much as the Russians and the Americans, helped invent it.

This book is, then, a work that may be read, in part, as a contribution to the literature on the origins of the cold war. Weiler agrees with those "revisionist" historians who argue that the Soviets were not alone responsible for postwar tensions. He shows that, even before a Soviet threat was apparent, the British Labour government and its trade-union allies were determined to uphold the empire, oppose revolutionary movements, and maintain the traditional balance of international power. More specifically, Weiler has combed the documents at the Public Records Office in Kew and the files at the U.S. National Archives to amass a mountain of evidence that suggests that, from the 1930s onward, the TUC cooperated with the British government to promote "responsible" colonial trade unionists who would not question British imperialism; worked, after the war, with the American Federation of Labor (AFL) to sabotage the World Federation of Trade Unions (WFTU), which had been established to maintain the wartime cooperation between socialists and communists; cooperated with the postwar Labour government to destroy independent trade unionism in Greece and West Germany in order to defeat communists in those countries; used the cold war (as did the British government itself) as a convenient smoke screen for its own conservatism and to justify red-baiting its critics; and, finally, participated in covert manipulation of the media both at home and abroad in order to attain these and other related goals.

Weiler aims, however, not so much to portion out blame for the cold war as to uncover the political background of British actions during it, thus making more complete our understanding of a crucial epoch in modern history. For example, in one of his best chapters, he shows that, although government and trade-union leaders condemned the London dock strike of 1949 for being communist inspired, its origins were actually much more complex. Communists were among the leaders of the strike and may have hoped it would embarrass the Labour government and undermine its economic recovery program. Nevertheless, Weiler convincingly traces the dispute and its subsequent expansion to AFL "poaching" in Canada and the attempt of some Canadian ship owners to smash a militant seamen's union there; then, after the strike had spread to London, to the British government's attempts to answer, once and for all in its own favor, the question of "who rules dockland"; and, finally, to traditional docker loyalties and customs. In short, Weiler adds a political dimension to our knowledge of the cold war that is lacking in most studies confined, as they usually are, to the diplomacy of the period.

But why was a socialist government, and why were trade-union leaders, so willing to side with employers in labor disputes and with American free enterprisers around the world? Quite rightly Weiler points here, with some sympathy, to Britain's desperate economic

situation in the postwar world and to the Labour government's dependence on U.S. handouts and its consequent reluctance to offend its patron. But, in this regard, his book is best understood within a framework established by historians who have criticized the Labour party from the Left. David Coates, Ralph Miliband, David Howell, and John Saville, among others, locate Labour's conservatism in an attitude that dates back to the movement's earliest days. According to these historians, "labourism," as Saville calls it, is embedded in an understanding of socialism and how to get it, which triumphed in Britain during the first two decades of the twentieth century, when figures like Sydney Webb, Ramsay MacDonald, and Arthur Henderson finally stamped the Labour party in their own image, driving militant members of the Independent Labour party, Marxists, and syndicalists to the periphery. Weiler agrees. He extends the argument, however, by showing that "labourism," at least during this period, characterized not only the party's outlook on domestic matters but also its cautious foreign policy.

Here a related theme emerges. When the government carried out its "labourist" policies both at home and abroad, it received unswerving, indeed nearly automatic, support from the leaders of Britain's major trade unions. Along with Keith Middlemas, Leo Panitch, and others, Weiler holds that, before the advent of Margaret Thatcher, labor (meaning here the trade union movement, not the party) had been "incorporated" into the British state. Over the years it had established the right to be consulted by whichever government was in power on all matters pertaining to its interests. But, in gaining access to state power, it came, at least in part, to identify with the state and to share the views of the state's permanent and elected officials. In an amusing aside, Weiler shows, too, that the process of incorporation was not all one way. During the early years of the cold war, the mandarins in the Foreign Office were not above proclaiming the virtues of social democracy. "Democratic socialism," said one of them approvingly, "is communism's rival for the allegiance of the working classes all over Europe" (p. 197). Such sympathies would not have been broadcast during Lord Salisbury's day.

In sum Weiler sheds new light on the British Labour party and the cold war through diligent archival research and deep reading in secondary studies, including the theoretical work concerning Labour's essential character and the role of trade unionism in British society. In this fashion he provides a model for historians who believe that diplomacy reflects the history of domestic politics as well as the acumen of individual diplomats. And, obliquely, he suggests an approach to the current debate on the future of the Labour party in Thatcher's England. Socialists who wish to re-create the Attlee governments may want to think again after reading this important book.

JONATHAN SCHNEER
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JONATHAN SCHNEER. *Labour's Conscience: The Labour Left, 1945-51*. Boston: Unwin Hyman. 1988. Pp. xiii, 249. \$45.00.

In this provocative and exhaustively researched study, Jonathan Schneer makes a case for the centrality of the left wing of the Labour party during the 1945-51 government and for the enduring significance of its legacy. Challenging the view of Kenneth O. Morgan's authoritative *Labour in Power, 1945-1951* (1984) that the ministry of Prime Minister Clement Attlee enjoyed pervasive support for its policies, he argues that these years were marked by growing disenchantment among left-wing elements in the Parliamentary Labour party and constituency organizations. Although it is difficult to measure the extent of disaffection, Schneer suggests that at least one hundred constituency bodies registered protests over official policies and that as many as one-third of Labour M.P.'s had serious misgivings about the pro-American stance of Foreign Secretary Ernest Bevin. In addition, Schneer reveals that discontent was rampant within the trade union movement, the rock on which Labour's fortunes rested, most notably over the issue of wage restraint.

Initially optimistic that Labour's decisive electoral victory in 1945 heralded the advent of socialism, left-wing dissidents soon became apprehensive about the drift of government policies. Instead of continuing to cooperate with the Soviet Union and revolutionary movements abroad, as Labour leaders had advocated, Bevin pursued a course almost indistinguishable from that of Conservative predecessors, much to the delight of the American State Department. Labour's left wing, convinced of the need to establish a democratic socialist third force to mediate between the two superpowers, denounced the division of the world into hostile blocs, but its plea fell on deaf ears as the Labour party eagerly embraced the concept of an anti-Communist Atlantic alliance. While Attlee's government implemented promises to nationalize vital industries, its leaders derided proposals for industrial democracy that would have given workers a direct share in management, preferring to rely on public corporations that effected little redistribution of economic power.

Idealistic about prospects for socialism and postwar reconciliation, the Left proved to be as misguided in the 1940s as it had been in the 1930s. Hopes for socialist reconstruction in Europe were irretrievably shattered by the Communist coup in Prague, which isolated fellow travelers in Britain and convinced most of the "soft Left" that there was no alternative to a Western union, subsidized by the United States, as a bulwark against Soviet aggression. Having earlier castigated the Americans for imperialistic aims, most of the Left welcomed the Marshall Plan as the salvation of Europe and, ironically, as the financial means to enact socialist economic measures.

Despite his own sympathies for their objectives, Schneer is obliged to concede that the Left was frequently its own worst enemy. Lack of unity, most

pronounced in the rifts between moderates and Communist sympathizers during 1947-48, ensured that protest was invariably ineffective. It was muted as well because of appeals to loyalty from party leaders, since few dissidents were prepared to carry their objections to the point of voting against the government and jeopardizing its survival. Most of the figures that Schneer identifies, middle class in background and first elected to Parliament in 1945, were political novices, out of touch with the trade union movement and no match for leaders whose domination of both the party executive and the annual conference guaranteed their security. Lack of political stature militated against their influence, but the party's recourse to disciplinary measures, including the expulsion of the most intransigent rebels, suggests that dissent was not taken lightly. Not until 1951, when Aneurin Bevan, the most prominent left-wing cabinet minister, resigned over the implementation of health service charges to pay for rearmament, did the Left mount a credible threat to the Labour party's tenure in office and, even then, it did so only because its majority had been drastically reduced in the general election of 1950.

This is an impressive work of scholarly rehabilitation, but, given the paucity of the Left's accomplishments, Schneer's contention that its role in these years "constitutes a chapter of great importance" (p. 211) in the party's history seems as arguable as his claim that its alienation from the Labour mainstream produced "an extraordinary burst of creativity" (p. 2). To be sure, its members sought to modify policies, not to gain power; they regarded themselves, Schneer aptly observes, as "the keepers of Labour's conscience rather than as an alternative leadership of their party" (p. 222). Their legacy, however, might appropriately be viewed more as internecine squabbling over ideology, which continues to plague the party, than as clearly articulated prescriptions for an eventual socialist commonwealth.

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JOHN HUTCHINSON. *The Dynamics of Cultural Nationalism: The Gaelic Revival and the Creation of the Irish Nation State*. Boston: Allen and Unwin, in association with the London School of Economics and Political Science. 1987. Pp. xiii, 343. \$34.95.

John Hutchinson's monograph is a useful contribution to the literature on the origins of the Irish nation state and the character of Irish political identity in the twentieth century. In this highly theoretical study, the author maintains that recent scholars of nationalism have underestimated or ignored the significant role that cultural revivalists have played in the development of separatist nationalist movements in modern European history. This study further substantiates the crucial role of cultural nationalism in defining a separatist Gaelic and Catholic Irish identity that helped prepare the way for the partition of the island and for the

severance of the constitutional and political links between southern Ireland and Britain after World War I.

According to Hutchinson's thesis, cultural nationalism influenced political developments once it had successfully completed a three-staged evolutionary development, categorized as preparation, crystallization, and articulation of sociopolitical goals. In the first stage, isolated scholars, poets, and intellectuals, inspired by eighteenth-century cultural theorists such as the German J. H. Herder, gave definition to the idea of their nation as a culturally distinct, living, and autonomous community. The crystallization stage brought the establishment of institutional structures such as national academies, newspapers, and language revival groups committed to recapturing the community's unique heritage. Finally, once there was mass acceptance of existence as a distinct people, the community's political elite were encouraged or compelled as the price of political survival to develop political goals that reflected the distinct national identities formulated in the earlier stages. At this juncture, cultural revivalism became politicized, often as a necessary defense against cosmopolitan and alien influences deemed threatening to the national spirit. It is during this stage of sociopolitical articulation that Hutchinson sees the links between cultural nationalism and political nationalism as most crucial.

By focusing this theoretical model on three periods of intense cultural revival in Ireland—the mid-eighteenth century, the 1830s, and the Gaelic revival of the 1880s and 1890s—Hutchinson demonstrates that cultural nationalism helped mold an Irish ethnic identity that was more comprehensive, forward looking, and positive than the Anglophobia that characterized early nineteenth-century national feeling. In addition to providing symbols that legitimized Ireland's claim to self-determination, cultural nationalism was used by pragmatic political nationalists to unify socially and economically diverse constituents and recruit mass support for political programs aimed at democratization and, ultimately, national independence. Cultural revivalism is seen, therefore, as an important factor in the modernizing process rather than as an archaic and romantic throwback to the past.

This study is unique in its adoption of sociological constructs to analyze the relationship between cultural nationalism and the rise of political nationalist movements. The class origins and personal motivations of leading Irish cultural revivalists (Charles O'Connor, George Petrie, Thomas Davis, Eoin MacNeill, Douglas Hyde, W. B. Yeats) are examined in detail, and the typical revivalist is shown to have experienced a personal identity crisis as a result of alienation from the legitimate state (Britain), his or her particular social class (Anglo-Irish ascendancy, the rural poor), or, in some cases, the contemporary nationalist political elite (Irish Parliamentary party). Identification with the perceived historic community provided the alienated with an emotional satisfaction and often converted the indi-

vidual into a dedicated apostle of some aspect of the Irish cultural revival movements.

Hutchinson's most extensive analysis focuses on the Gaelic revival of the 1880s and 1890s, an era in which alienation spread beyond the scholarly few to a critical percentage of the literate members of Irish society. This alienation was often rooted in an intellectual revulsion against accelerating anglicization and secularization as well as in the frustrations that limited social mobility produced in the well-educated but politically excluded and patronage-deprived Irish lower-middle classes. Resolution of those inner conflicts led individuals such as Arthur Griffiths and D. P. Moran to formulate political and economic programs for national self-sufficiency and the regeneration of the entire community that were more radical than the traditional liberal democratic Home Rule solution advocated by the Irish Parliamentary party, a solution that the author defines as rationalist, statist, and assimilationist.

Hutchinson's extensive data on the blocked social mobility common to many young Gaelic enthusiasts represent an important contribution to an understanding of the forces that shaped Irish separatist thinking. It was this generation who, possessing visions of a distinctive communal and national life, emerged among the vanguard of the separatist Sinn Féin and republican movements from 1912 to 1921 and who gave priority to Gaelic and Catholic aspects of Irish identity in postindependence Ireland. The commentary on lower clerical involvement in the Gaelic language and sports movements as well as on the opportunities that the Parnellite split provided for propagating revivalism in general provides useful insights on the waning political dominance of the Irish Parliamentary party just prior to 1914.

Although the personal histories of the leading Irish cultural revivalists and nationalists have been previously available, Hutchinson's study has merit in the theoretical comparisons and contrasts he draws between cultural revivalists at different stages in the evolution of Irish cultural nationalism. The distinctions noted in the third revival between the motives and goals of elitist and popularist cultural revivalists, such as those that distinguished Yeats from Moran, further our understanding of the complexities and tensions of Irish politics in the early twentieth century. Finally, this study demonstrates that the visions of an ideal society that cultural nationalism provided left a legacy of expectation that was not easily fulfilled once former cultural nationalists were faced with the responsibilities of power as practicing politicians.

Hutchinson's achievement is to have placed the Irish experience in a broad theoretical context that will invite the interest not only of scholars of Irish political history but also of those involved in comparative analyses of nationalism in modern Europe.

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MARCEL LACHIVER. *Vins, vignes et vignerons: Histoire du vignoble français*. Paris: Fayard. 1988. Pp. 714. 190 fr.

Not since the trailblazing publications of Roger Dion in the 1950s has a work of such magnitude appeared on the history of wine and wine growing. Since then the subject has attracted a host of academics, chiefly geographers and historians, turning out their weighty (in both senses) doctoral dissertations. All of those studies, however, are limited in space and time.

Marcel Lachiver, a professor of history at Nanterre, is one of that group, having published his thesis, *Vin, vigne et vignerons en région parisienne du XVIII^e au XIX^e siècle* (1982), a 957-page colossus. He, however, has subsequently followed Dion and now gives us an impressive history of the entire French wine industry over two thousand years, a remarkable synthesis.

Lachiver begins with the Gauls and the Franks, during whose long reign the wine industry migrated from the Mediterranean to the north, where it became fragmented into numerous small viticultural localities. They produced mainly white wine because only white grape varieties could attain at least partial maturity in the cold, humid climate. The main reason for grape cultivation under such hostile conditions was the liturgical need of clergymen for wine. Churchmen of every order, therefore, planted vineyards, and over time they adapted varieties first to climate, then to soil. Their efforts produced a viticultural and vinicultural golden age from the eleventh to the thirteenth centuries. Lachiver attributes that expansion to the tripling of population in the north and to the rise of towns as markets. Wine, although still required for holy ritual, became a commodity produced by all classes. What kind of wine? Admitting that wine was healthier than water and offered calories to hungry stomachs, it was surely acidic and rough, acceptable only to the medieval digestive system. The Black Death and Hundred Years' War ended the golden age as markets declined.

In his section on the next three centuries, Lachiver has profited from an abundance of data about vine culture and wine making. He traces the emergence of innovative techniques that made it possible for growers in the Bordeaux, Burgundy, and Champagne regions to produce wines that satisfied not only the thirst of the rich but also their increasingly refined sense of taste. Now red beverages attained prominence. Their quality improved, thanks to clerical and lay growers as well as merchants who began to classify wines according to growths. Apart from those expensive nectars, however, the general level of quality declined from the 1730s to the 1950s. The search for high yields in order to satisfy the urban lower classes was the cause of that unhappy trend. France turned out the best and the worst.

Lachiver goes into considerable detail about the reverse geographic shift of vineyards from north to south, a result of the declining cost of transport by railroads and the phylloxera invasion that destroyed all the vineyards, including those of the north. The mid-nineteenth century was the turning point, and the

emergence of the vast vineyards of the lower south, spouting out cheap red wines, discouraged replanting above the Loire. But that red sea has brought a fall of prices, malaise, and persistent turbulence to the Midi. Vinicultural cooperatives have alleviated some of the problems. So has improved quality encouraged by the system of controlled appellation.

Lachiver's large volume is a tour de force, even a "Tour de France" viticole. It is based on his vast knowledge of the subject and his incredible control of detail. It is also written in an engaging style. His fifty-seven-page bibliography, limited to printed works, is impressive enough. His maps, tables, graphs, and illustrations add considerably to the text. There is only one aspect of the wine industry that is not fully discussed: in Lachiver's pages there are *vignes* and *vins* but not many *vignerons*. *Vignerons* are not entirely absent, and Lachiver, also a demographer, offers some useful information about their marriage patterns, but little else. Otherwise the book is excellent. More than that, it is definitive.

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ANDRÉ E. GUILLERME. *The Age of Water: The Urban Environment in the North of France, A.D. 300-1800*. (Environmental History Series, number 9.) College Station: Texas A&M University Press. 1988. Pp. xv, 293. \$29.50.

This is an unattributed translation of André E. Guillermé's *Temps de l'eau* (1983). The author, an engineer rather than an historian, provides a provocative thesis concerning changes in man-made waterways in eighteen cities of the Paris basin.

In establishing their cities after the third century, the Romans in effect reconstituted Celtic *oppida* by diverting rivers with names sacred to local deities to serve as moats. The Christians responded by adopting saints whose attributes and names preempted those of the Celtic gods. Although this linkage is plausible, to call it "a form of theological urbanization" (p. 15) with minimal strategic significance is strained. In the ninth century, however, much attention was paid to town fortifications. Although moats followed urbanization elsewhere, they preceded it on the lower Seine, where they made the ground firm enough to support large fortresses by draining marshes near the rivers. Even when settlement continued at a Gallo-Roman site, it usually moved away from its previous center, propelled by a "new ideology of feudalism" (p. 47).

Princely courts played a critical role in the first medieval urban growth. Governing authorities diverted water into the cities to provide drainage for gardens and palaces and particularly to permit the construction of mills. Even in the cities nearest the coast, harbors were secondary to the need for mills, which required running water and were normally

located on streams that drained away from the town. Marshes, which the Romans had avoided, were an important component of virtually all medieval cities. As the bogs were drained by the major streams where the mills were located, artisans who needed standing water, such as tanners and textile workers, moved onto the reclaimed land and linked their work places to the canals by smaller conduits. During the twelfth century, the ratio of land area to meters of waterway in the larger cities of the Paris basin was comparable to that in Venice during the thirteenth century.

Guillaume denies that the cities were seriously polluted before 1300, for urban canals and moats drained away from the cities' centers. But the fourteenth century brought a rainier climate and warfare. The moats were widened disproportionately for defensive purposes and became stagnant pools rather than runoff channels. The armies and the cities themselves desolated the suburbs, which thus reverted to fetid swamps.

The cities modified their industrial techniques to fit the changed ecological situation. Most water mills, which had been in the suburbs, were destroyed in the wars and were not rebuilt. Whereas medieval textile industries had specialized in woolen cloth, those of the modern period used vegetable fibers that required soaking in elements rarely used in medieval industry, such as animal wastes. A manufacturing zone of marshes and man-made canals surrounded the early modern cities. Guillaume attributes that condition to a "new mentality," for in its fascination with death and putrefaction, Renaissance civilization was a "culture of the fungal" (p. 174).

Beginning in the eighteenth century, chemists discovered how to alter substances by using minerals rather than organic putrefaction. The old urban water network was no longer useful. The moats and conduits were filled in and the ramparts torn down. The "age of water" is thus a bipartite phase of urban history: between 950 and 1300, running water provided the energy for the technology used in urban crafts; and between 1300 and 1800, urban chemical activities were based on the principle of slow maceration of organic matter in stagnant water.

This is a stimulating book whose theses will be tested for the history of urbanization in other regions. Guillaume admits that they seem to work less well for England and the Netherlands. But the book would have been better if the author had confined himself to the subject of urban waterways. He clearly wants to fit the city into an ecological and structural scheme. Unfortunately, he has read too many of the wrong syntheses, and the result is a distracting series of abstractly pedantic howlers. Furthermore, in a book as dependent as Guillaume's on discussions of urban topography, it is inexcusable to have maps that are blurred in places to the point of illegibility and that are replete with symbols for which no key is provided.

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RALPH E. GIESEY. *Cérémonial et puissance souveraine: France, XV^e–XVII^e siècles*. (Cahiers des Annales, number 41.) Paris: Armand Colin in cooperation with l'Ecole des Hautes Etudes en Sciences Sociales. 1987. Pp. 170. 80 fr.

In recent years a good many historians of France have pursued the conception that Ernst Kantorowicz identified as that of the "king's two bodies." Of those historians, Ralph E. Giesey, Kantorowicz's student, assistant and friend, was by far the earliest.

This densely written study, originally presented as lectures at the Raymond Aron Institute of the Ecole des Hautes Etudes en Sciences Sociales, comprises mature reflections and further hypotheses of a distinguished scholar on a subject that has claimed his attention for much of his career: the relationship of French juridical theory to French monarchical ceremony. Here the main themes and propositions of his *Royal Funeral Ceremony in Renaissance France* (1960) are reworked, given new emphasis, and supported by additional evidence.

Over the course of the fifteenth and sixteenth centuries, French royal funeral ceremonies evolved in the direction of symbolizing the perpetual dignity of kingship—the "mystical body" of the king as distinguished from his person—and its identity with the nation. Giesey emphasizes two aspects of the trend toward the "classic" ceremonial form: growing emphasis on the royal effigy as representing the "living king," or the "king's justice," rather than substituting for the dead body, and, at the interment, ritual words and gestures evoking the continuity of sovereignty, even as the site of its incarnation changed from the body of the departed king to that of his successor.

In the present book Giesey makes new use of detailed accounts of funeral ceremonies of the dukes of Lorraine and sharpens earlier comparisons. The forms in which the passing of French kings was celebrated differed from ducal rituals in just those ways that royal authority's association with "eternal France" contrasted with the house of Lorraine's dynastic particularism.

Jean Bodin (on whom Giesey has recently written a notable commentary) broke with whatever tendencies there had been in French constitutional theory to consider actual kings as persons subject to secular conceptions of enduring law, just as the events following Henri IV's death presaged termination of the ceremonies that had marked these trends. By 1610 French monarchical ceremony, like French public law, was veering away from any parallel development with England. The ceremonial "coronation" of Louis XIII (as François de Malherbe put it), on the day following his father's death on May 14, 1610, brought the inaugural function of French royal funerals virtually to an end. Over the reign of Louis XIV, the old public ceremonies were abandoned, and sovereignty was identified with the person of the king. Every aspect of his life in court became ceremony.

Difficulties of verification arise when trends are iden-

tified in a time series with as few cases as that of French royal funerals from 1400 to 1610. Thus, Gieseey is "a bit distressed" to see a "regression," in 1559 and 1574, from the vigor with which the notion of the king's two bodies had been ceremonially represented in 1547 (p. 108). Again consider the expected trend toward ritual graveside cries in which the impersonal "Vive le Roy" precedes the personal "Vive le Roy X." Gieseey plausibly believes the sequence to have been an intention at the funeral of François I, not followed in the event but followed for his successor, Henri II. The reason it was not followed for Charles IX in 1574 is not investigated, nor is an explanation suggested for why it was followed for Henri IV, even though the rationale for this sequence of *vivats* had disappeared. Discussion is entirely omitted of funerals without effigies, such as the unceremonious one of François II and the funeral, too long delayed, of Henri III. Yet, in such a restricted field, cases that deviate from postulated trends may have much to tell us.

In sum, one might suggest that far from apologizing for a laudable attention to details—in which not God, to be sure, but "the deepest meaning is hidden" (p. 23)—this very discriminating scholar could expand his fascinating little book with even greater advantage to his readers.

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HAROLD A. ELLIS. *Boulainvilliers and the French Monarchy: Aristocratic Politics in Early Eighteenth-Century France*. Ithaca: Cornell University Press. 1988. Pp. xii, 283. \$34.50.

Henri de Boulainvilliers (1658–1722) was an aristocratic reactionary who believed wholeheartedly in genealogical consciousness, the inherited legacy of virtue of the French nobility. Boulainvilliers undertook the rehabilitation of French feudalism, that "masterpiece of the human mind" (p. 5), a project already regarded as quixotic by his contemporaries. Although his involvement in practical politics was unsuccessful, he had wide interests in religion, history, and politics. He wrote much but published little in his lifetime, preferring to circulate his manuscripts to a small, select audience: first, in the 1690s, to the circle of the duke of Burgundy, the would-be enlightened heir to the throne; then, after Louis XIV's death in 1715, to the Regent Philippe d'Orléans.

In Boulainvilliers's works historians have found glimmerings of constitutionalist thought, for he objected to despotic features of royal absolutism. Boulainvilliers was the subject of a lengthy work by Renée Simon (*Henry de Boulainvillier*) in 1941. Now, with a fresh look at the sources, Harold A. Ellis has produced a study limited to Boulainvilliers's historical and political writings on the French monarchy.

Ellis faced formidable obstacles. It is hard to put

undated manuscript memoranda and histories in the context of political events. Furthermore, it is difficult to tell what audience Boulainvilliers had in mind, whether they read his work, and what effect it had on them. Ellis steers a middle course of interpretation: he thinks Simon was too favorable to Boulainvilliers, while André Devyver, in *Le Sang épuré* (1973), was too harsh in calling Boulainvilliers a racist. The Boulainvilliers that emerges in Ellis's study is admittedly a reactionary but "not the ideological monster" (p. 209). He wrote with the intention of counseling the prince. His defense of the interests of the nobility as a whole required subtle shifts in position. In Burgundy's circle, Boulainvilliers sought to limit absolutism through the proper education of the prince and planning for his reign. Yet in the Regency, he was willing to use the authority of the monarchy against the dukes and peers as well as against the judges of the Parlement of Paris. Ellis provides a close reading of Boulainvilliers's texts and indicates his extensive historical borrowings from Jean Le Laboureur, François Eudes de Mézeray, and Louis du Four, Abbé de Longuerue. As a historian, Boulainvilliers certainly had shortcomings. Ellis sees them in the dusty medieval histories and prefaces to the *État de la France*, Boulainvilliers's collection of the survey done for the duke of Burgundy, although Ellis could have made more of Boulainvilliers's slanted editing of the intendants' reports.

Ellis expressly excludes Boulainvilliers's works on philosophy, astrology, ancient history, and the history of religion, as well as some striking political writings attributed to him by other scholars, such as a memorandum to the regent urging the creation of "companies of commerce" to collect taxes and provide public welfare benefits. Five years after Boulainvilliers's death, these works appeared in Dutch and English editions to which editors and printers may have added copy. Thus, their authenticity can be questioned. In a detailed appendix, Ellis proposes bibliographical revisions "on the basis of what one knows about his ideological and intellectual character" (p. 245). Ellis's painstaking labors may not convince everyone, however, since the image of Boulainvilliers depends heavily on which works one reads.

On the whole, Ellis's analysis of Boulainvilliers's political works is solid and sure-footed. It will be welcomed by scholars in French history and political theory.

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ROBERT LOUIS STEIN. *The French Sugar Business in the Eighteenth Century*. Baton Rouge: Louisiana State University Press. 1988. Pp. xi, 185. \$27.50.

This book should appeal to a wider audience than its title suggests. In fewer than two hundred pages, Robert Louis Stein offers a lucid summary of current knowl-

edge about a wide variety of topics. These include the French slave trade, the plantation economy in the West Indies, the methods of financing and outfitting overseas expeditions, commercial practices within France, as well as the production, refining, distribution, and consumption of sugar. Stein reiterates some of his findings from earlier books and articles on the slave trade, but he also has undertaken fresh archival research and scoured the abundant secondary literature relating to the topic.

His conclusions will not surprise specialists, but this book provides a handy survey that ties together numerous strands of French eighteenth-century economic history. Stein explains, for example, how the production and trade of sugar led businessmen to combine both old and new commercial practices in a sort of transitional form of enterprise. Old elements included the prominence of merchant middlemen in all aspects of the trade and the importance of family partnerships and networks. A new feature was the wider gulf between capital and labor that emerged in the colonial plantations and in the large refineries in France. The slaves in the Caribbean and the free employees in France composed a more specialized and disciplined work force than had been customary in agriculture and workshops.

Numerous other themes appear in the book. Stein repeatedly demonstrates how the interests of West Indian planters and French merchants clashed. This friction resulted from several sources: planters' non-payment of debts, sharp restrictions on the refining of sugar in the colonies, illegal trade between French colonists and foreigners, and objections by French liquor producers to the sale in France of West Indian rum, among other factors.

Stein charts the impressive growth of the French sugar trade through the century but also deftly points out failures and missed opportunities. Despite the fact that the amount of sugar entering France increased threefold from 1730 to 1790, the trade was always a risky one and yielded only modest profits for the great majority of those involved in it. France dominated the European sugar trade; most of the raw and semi-processed sugar that came from the Antilles was reexported to other countries. But, for a variety of mostly fiscal and political reasons, the French refining industry never grew to more than modest proportions. A high percentage of the profits made from refining therefore went to the Dutch and German manufacturers who purchased their raw products from France. What was worse, the sugar that left France continued to be transported largely in Dutch ships.

Stein is relatively sympathetic to the royal government and its regulatory policies. Although the crown generally tried to promote the interests of the nation as a whole, it was too weak to overcome the petty jealousies and animosities of competing towns and business firms. Cities and individuals clamored for the preservation of their particular tax privileges or monopolies. The result was that merchants and refiners generally

aimed at local or regional markets rather than a national one.

In short, this book illustrates how the sugar business must be viewed as but one part of the vast canvas of changes and continuities that constituted the Old Regime.

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J. F. BOSHER. *The French Revolution*. (Revolutions in the Modern World.) New York: W. W. Norton. 1988. Pp. lxi, 353. \$24.95.

J. F. Boshers's latest book is an attempt to address the negative tone of the revisionist interpretation of the French revolution. Like many of his postrevisionist contemporaries, Boshers not only eschews class-based explanations of the revolution but also relegates social explanations to the sidelines. Instead, he argues for the importance of political culture, personalities, bad luck, and chance. Thus, Louis XVI was brought down because he chose not to defend the monarchy through the traditional authoritarian methods of censorship, arbitrary arrests, bankruptcies, and so on. This well-meaning, decent, and liberal king tried to accommodate a political opposition that by the 1780s included most thinking people in the realm. The Estates-General and, subsequently, a liberal constitution came into being because everyone, including the king and his like-minded ministers, desired an end to absolutist practice.

Except for the positive characterization of Louis XVI, this interpretation is in line with much current thinking, but Boshers parts company with revisionists such as François Furet who argue that the Terror of 1793 was contained in the principles of 1789. Instead, the Terror emerged from the atavistic violence of the common people who were themselves antiparliamentary, envious, gullible, and ignorant. A minority of Jacobin ideologues rationalized these sentiments, and together they overwhelmed the liberal constitution whose very principles required that these dangerous men be tolerated. The real turning point in the revolution was thus the fall of the monarchy on August 10, 1792, which occupies a central place in the narrative, rather than the fall of the Bastille three years earlier, which gets only a few lines. Yet even the terrorists succumbed to Napoleon Bonaparte who knew that the nationalism of the French was stronger than their liberalism.

In many ways, this book is elegant, thought-provoking, engaging, and humorous by turns. Students will find parts of it excellent and useful at once: the lengthy date line, the potted biographies of major figures, the discussion in the last two chapters of the significance of the revolution in French history, the exceptionally clear exposition of reforms in the civil service, the unique analysis of the politics of high finance in the Old Regime and the role of court connections in appoint-

ments and royal policy, and so on. Specialists should be disconcerted by the citations from many classic historians apologizing for the lynchings and judicial travesties of the period.

Still everyone should realize that defining the revolution as the fall of the monarchy and the Terror as mass violence is the basis of Boshier's interpretation. An alternative approach that could define the revolution as an assault on despotism and privilege and the Terror as a civil war reopens the way to a social interpretation of sorts. Perhaps this opening would even result in a more charitable view of the common people.

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ROBERT HOWELL GRIFFITHS. *Le Centre perdu: Malouet et les "Monarchiens" dans la Révolution française*. Grenoble: Presses Universitaires de Grenoble. 1988. Pp. 277. 125 fr.

The importance of Robert Howell Griffiths's ably argued and convincing thesis must be measured by its departure from the traditional interpretation of the role of the *Monarchiens* in the French revolution. In that view, the name applies primarily to those moderate men who supported Jean-Joseph Mounier in 1789 in the vain attempt to limit the power of legislative assemblies by bicameralism and an absolute royal veto. The name, however, was also used by Mounier's successors, who founded two ephemeral monarchical clubs in 1790–91. The *Monarchiens* are thus commonly seen as a few moderate royalists, destined to futility.

Griffiths's picture is very different. After an interesting distinction between moderation "of position" (that is, compromise between two ever-evolving extremes) and "of policy" (that is, the constant exposition of a fundamentally distinct proposal), he firmly identifies the *Monarchiens* as men advocating a positive alternative to both Jacobinism and "aristocracy." Seeing the principal champion of this alternative as Pierre Victor Malouet, Griffiths shows that the term *Monarchiens* was not coined until Malouet and Antoine de Clermont-Tonnerre acted together in 1790–91. Further, he argues that the previous association of Malouet with Mounier was essentially transitory, even though both men opposed the concentration of all authority in a unicameral assembly, for, where Mounier and those whom Griffiths calls constitutionalists hoped to effect a judicious division of executive and legislative power, Malouet and the *Monarchiens* believed in a unitary state in which the king would have both executive and legislative power while the assembly would be merely consultative. Behind this conception Griffiths sees the long struggle of prerevolutionary ministers to establish an efficient and enlightened centralized administration against the opposition of innumerable particular interests and intermediate bodies. When more fully developed, as it was during Malouet's years in exile, this *étatiste* solution to France's dilemma included extensive

plans for social welfare, but Griffiths asserts that even in 1790–91 the conception was so potentially popular that the Right and the Left combined to thwart, discredit, and destroy its proponents.

Writing with commendable clarity, Griffiths substantiates his interpretation despite the difficulties involved in analyzing the ideas of an unstable group of men in the changing circumstances of ten momentous years. The ideas of the exiles are rightly seen as an integral part of the whole, but Griffiths seems unduly concerned with distinguishing the *Monarchiens* from those royalists who were committed to the restoration of an aristocratic order, and he condemns both the royalists and the Jacobins without adequate representation of their views.

This book is nevertheless a valuable contribution to understanding the French revolution. It also illustrates the refreshing historiographical development in which new attention is being given to the political realities of the initial years of the revolution. In Griffiths's work, appreciation of the importance of prerevolutionary administrative reforms and of the increasing social power of the notables and *propriétaires* is again apparent. This is not, of course, to say that we need accept this stimulating author's disregard both for the liberties of the old order and for the democracy of the revolution.

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BERNARD MENAGER. *Le Napoléon du peuple*. (Collection Historique.) Paris: Aubier. 1988. Pp. 445. 155 fr.

Only a generation ago, historical scholarship on Bonapartism, although it was one of the key political and ideological currents of modern France, could be summed up by mentioning just a few works; citation of Albert Guérard on the Napoleonic legend, an essay by H. A. L. Fisher, some sidelights of André Siegfried, and a penetrating chapter by Charles Seignobos in his volume of Ernest Lavisse's *Histoire de la France contemporaine* (1920–22) virtually completed the list. Today so much has been written by so many that much of Bernard Ménager's study of the origins and decline of Bonapartist feeling among the French common people amounts to an effective synthesis of the work of others. By now one wonders how much more light can or should be shed on the whole subject.

Author of a massive thesis on the political life of the Nord, 1851–77, Ménager here offers an account both of the *mentalité* of popular Bonapartism and of its social and geographical matrices over a *longue durée* from 1815 to the death of the last real pretender in 1926. His chapters are of unequal originality. He has effectively scoured documents, primarily from the BB-Justice series at the Archives nationales, to find out what sort of people were prosecuted for uttering Napoleonic "seditious cries" under the Restoration, discovering, not surprisingly, that a majority were artisans of the

humblest trades. His account of the growth of the Napoleonic legend under the July Monarchy, by contrast, relies mainly on secondary works to tell a familiar tale.

Probably the most valuable part of the book is comprised of the three middle chapters that focus on the evolution of popular Bonapartism under the Second Empire, a regime that faced the probably insoluble dilemma of reconciling the popular vision of the Bonapartes as champions of democratic change with the insistence of the upper classes that they be bulwarks of a hierarchical society. Skillfully synthesizing the vast literature of regional political geography with Vincent Wright and Bernard Le Clère's collective portrait of the imperial prefects, Ménager provides a virtual department-by-department account of the changing political fortunes of Bonapartism from 1851 to 1870. Although his method of analysis owes more to the impressionistic school of French political geography than to statistical rigor, he effectively contrasts the evolution of eastern, progressive, urban France away from Bonapartism between 1851 and 1870 with western, conservative, rural France's growing affinity for it.

For his final chapters depicting the ebbing of popular Bonapartism after 1870, the author relies heavily on the works of earlier authors, my own among them. Having scored a respectable comeback after Sedan, Ménager agrees, Bonapartism was doomed by its leaders' obsession with aligning themselves with the conservative monarchists, by quarrels among rival pretenders after the death of the prince imperial in 1879, and, above all, by the modernization of the French countryside, where increasing literacy and the rural exodus combined to undermine the kind of village society that had nourished the Bonapartism of the peasants, by 1870 the movement's principal supporters. Having begun as an urban, popular, and even "leftist" challenge to the Restoration, Bonapartism ended over a century later as the credo of another largely urban minority, a movement of militant middle-class conservatism that metamorphosed at last into the proto-fascism of Pierre Taittinger's *Jeunesses Patriotes*.

Ménager provides a stimulating survey of this protean force so important to modern French politics, ending by drawing the obligatory parallels with Gaullism. Unfortunately, the book is marred by an inordinate number of spelling errors. English names have always been a Gallic stumbling block, so it is not surprising to find Chislehurst—Napoleon III's last home near London—repeatedly rendered as "Childehurst." More unusual is the author's failure to spell correctly not only the names of prominent Bonapartists, such as Baron Eschasseriaux and the duc de Feltre, but also the names of fellow historians, such as Maurice Agulhon and André-Jean Tudesq. That the book has reached reviewers with such mistakes uncorrected is not only a reproach to the publisher. For the reader cannot help wondering, for example, whether

Ménager's appended tables showing the Bonapartist percentages of plebiscitary votes are similarly flawed.

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BERNARD DE BRYE. *Un Evêque d'ancien régime à l'épreuve de la Révolution: Le Cardinal A. L. H. de la Fare (1752–1829)*. Foreword by DANIEL ROCHE. (Université de Paris I, Histoire Moderne, number 16.) Paris: Publications de la Sorbonne. 1985. Pp. 319. 195 fr.

Cardinal Anne Louis Henri de la Fare has never been the subject of a complete biography. Several nineteenth-century hagiographies applauded his refusal to recognize the jurisdiction of the National Assembly, Napoleon, or the pope over the Gallican church but failed to explain his apparently contradictory views on church-state relations. In this first modern biography, Bernard de Brye makes no attempt to cover the cardinal's career through revolution and restoration. Instead, he discusses only de la Fare's early life through his exile in 1791 and summarizes his subsequent activities.

Whether or not de Brye intends to use this work as a basis for studying de la Fare's more interesting later years, it fits well into the efforts of the French clergy after Vatican Council II to reassess the clergy's revolutionary past using modern historical methods. Although de la Fare was not a dominant figure, his early career merits attention because he presented the opening sermon to the Estates General and later voiced conservative opposition to the Civil Constitution of the Clergy.

In this study de Brye substitutes sociopsychological concepts, statistical analysis, and textual reconstruction for narrative. First, the author depicts de la Fare's family and sociolegal background. He uses public archives and cites unnamed private collections but relies heavily on the memoirs of de la Fare's niece (the Baronne du Montent, published in 1904) to infer an unhappy home environment dominated by a powerful mother.

After discussing a cleric's preparation for high church office, the author deduces, from a statistical analysis of de la Fare's library, his youthful participation in what Bernard Plongeron calls the "Second Enlightenment." De Brye argues that the upper clergy's administrative experience enabled them to dominate prerevolutionary assemblies. But he provides few examples of his subject's activities as vicar-general in the intermediary commission of the Bourgogne Estates or in the 1787 Assembly of Notables; Vivian Gruder's recent study of the later body provides no evidence of de la Fare's allegedly leading role (Gruder, "Paths to Political Consciousness: The Assembly of Notables of 1787 and the 'Pre-Revolution' in France," *French Historical Studies* 13 [1984]). Finally, de Brye tries to infer the lost text of de la Fare's opening sermon to the Estates General by tabulating responses to it.

De Brye's attempt to present a modern biography of de la Fare partly succeeds. He shows the reader how the cardinal reflected the interests of his family, his class, and the *pays d'état*. Although de la Fare's religious beliefs lack clarity, the author shows how his opposition to revolutionary ecclesiastical reforms and his refusal in 1802 to abdicate were founded on his vision of a Gallican church loyal to both king and faith. Finally, de Brye demonstrates that de la Fare was not the nonentity depicted by liberal historians and that his pre-1791 activities merit attention.

Nonetheless, de Brye's book is unsatisfactory. After studying inaccessible document collections, the author could have presented a clearer impression of his subject's personality. Moreover, his several approaches lack integration; the reader is left with a fragmented picture and a wish that de Brye had both held a looser rein on his imagination and had provided clearer transitions between the different parts of his study.

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LLOYD S. KRAMER. *Threshold of a New World: Intellectuals and the Exile Experience in Paris, 1830–1848*. Ithaca, N.Y.: Cornell University Press. 1988. Pp. xii, 297. \$31.50.

For the poet Rainer Maria Rilke, Paris was a force that totally possessed those entering its space, requiring that their very existence and being be defined again. Lloyd S. Kramer's perceptive history of the years of exile in Paris of Heinrich Heine, Karl Marx, and Adam Bernard Mickiewicz, while not neglecting the agony and wounds of expatriation, affirms the indispensable contribution of their years in Paris to their original individual responses to France and Europe's passage from the limited revolution of July 1830 to the failed revolution of 1848. His history of the exile, ordeal, and opportunities experienced by Heine, Marx, and Mickiewicz sensitively and imaginatively recaptures the actuality and significance of their years as exiles in Paris, described and characterized as the capital of Europe.

The conventional memory of Heine's time in Paris is that he knew everyone of importance. A list includes, among many others, Frédéric Chopin, François Mignet, Adolphe Thiers, and Jules Michelet. The *papillon* features of Heine's character encouraged and encourages an exaggerated assessment of his Parisian contacts and friendships. Kramer does not linger, however, in the world of rumor and gossip. He succeeds in astutely presenting the biographical aspects of Heine's exile, and, more importantly, he persuasively establishes the significance of Heine's role as an informed witness to France and Europe's euphoric and despairing history in the years that began with the July Monarchy and ended with the June days of 1848.

One does not easily think of Marx as a displaced person, for he viewed the people, societies, and nations

of Europe as exiled from themselves. It was precisely the alienated societies and moral world of Europe that he sought to transform and liberate. Marx arrived in Paris at the age of twenty-five and was forced to leave in 1845. Kramer understands Marx as unhesitatingly prepared to use Paris as a place to make it possible for him to bring together refugee Young Hegelians to help fashion and spread the message of his projected *Deutsche-Französische Jahrbücher*. Brilliantly summarizing the consequences of Marx's Parisian education, including his close observation of the city's proletariat, Kramer underscores the importance of Marx's *Economic and Philosophical Manuscripts*. Of this work he concludes, "One might say that what Marx did in his Parisian texts was to historicize materialism and to materialize history" (p. 151).

In a manner completely different from that of Marx, but as decisive, Mickiewicz, the exiled Polish poet, exhaustively exploited the possibilities Paris so generously extended to him. A hero to the city, Mickiewicz is represented by Kramer as managing through his lectures, writing, and poetry to substitute for the mysteries of Paris and the glory of France the salvific mission of Poland to redeem France, Europe, and the world. It was an eschatology that Mickiewicz's admiring and affectionate friend Michelet found more perplexing than all of the problems encountered in his own vast historical studies. Kramer's interpretive history of these famous exiles communicates to his readers the awe and astonishment that Rilke associated with all visits to Paris.

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ADRIAN RIFKIN and ROGER THOMAS, editors. *Voices of the People: The Social Life of "La Sociale" at the End of the Second Empire*. Translated by JOHN MOORE. (History Workshop Series.) New York: Routledge and Kegan Paul. 1988. Pp. x, 330. \$65.00.

American academics are conversant with post-May 1968 French scholarship in psychology and philosophy. Less familiar is work in social history. Some projects well under way in 1968, such as Michelle Perrot's *Les Ouvriers en grève* (1974), bear the imprint of the events of May. In the late 1970s, a second generation of post-May social history developed. One step removed from the barricades, it is informed by a questioning of cultural and political authority (including that of the Left) and by a desire to reveal power and its subversion in society.

The five essays in this volume, originally published between 1975 and 1980, are among the best examples of this work. Each addresses the Parisian popular classes, focusing on the 1860s. The two articles authored or coauthored by Jacques Rancière were published in the now defunct *Les Révoltes logiques*; Alain Cottureau's study of *Le Sublime* introduced a republica-

tion of this work; the essays by Alain Faure and by Alain Dalotel and Jean-Claude Freiermuth formed the nucleus of a book on the popular meeting movement in Paris between 1868 and 1870.

Rancière's 1975 article (coauthored with Patrick Vauday) places the oft-evoked misogyny of French workers in the context of social struggle through analysis of statements by (male) worker-delegates to the 1867 exposition. The delegates saw intensification of capitalist production as posing two threats to workers: deskilling and employment of women. In the future, workers' associations should control technology; for the present, the home constituted the workers' first line of defense. Working wives allowed employer and state to encroach on the working-class bastion of the family. In any case, the delegates argued, women could achieve equality with men only through control of the home, never in the workplace. The delegates advocated the *femme au foyer* therefore not in terms of women's inherent inferiority, or even solely in terms of working women's effects on wages, but in the name of women's equality and workers' defense against capitalism.

By the time Rancière published his second article in this collection (1978), he had shifted his approach and come to interpret all ideologies—including those of the delegates in 1867—as mixtures of resistance and repression, liberation and discipline. Social practices could not in turn be fixed in place with terms like "bourgeois" or "radical." Rancière begins this provocative, innovative essay with an exploration of the ambiguous elements of July Monarchy popular culture, which neither directly reflected the workplace nor were the province of professional entertainers. He gives special attention to audience "performance" at the theater (especially in the first half of the century, when bourgeois and workers attended the same playhouses).

The July Monarchy and the Second Empire were not alone in trying to control the content and presentation of plays and songs. They were joined by a working-class political elite who argued that working-class culture had to combat bourgeois images of the uncivilized worker with a counterimage of respectability. Ironically, argues Rancière, in the process of delimiting worker culture, these militants created a new social status for themselves, neither worker nor bourgeois, analogous to that of the popular wineshop singers they criticized. During the first decades of the Third Republic, changes in working-class sociability, the commercialization of entertainment, the professionalization of artists, bureaucratic innovations, such as numbered theater seats, and technological developments, such as the phonograph and the cinema, worked to turn "a very small minority of those who could no longer sing at work into artistes, and the vast majority into spectators" (p. 73).

At the end of the Second Empire, Denis Poulot, the radical republican foreman turned employer, drew on his years of experience in Parisian machine shops to pen a "typology" of workers. Cottereau establishes a context for *Le Sublime* by attacking the static portrait of

Parisian industry as the preserve of skilled artisans and small masters. The highly capitalist Parisian industrial sector promoted the division of labor, deskilling, and outside contracting; it militated against close relations between employers and workers. In response, Poulot promoted social reforms and technological innovation aimed at moralizing workers and reducing their power on the shop floor.

Le Sublime is a richly detailed account refracted through what Cottereau calls Poulot's "bourgeois ethnocentrism." In Cottereau's hands, Poulot's effort to order and repress Parisian workers is made to elicit examples of workers' ironic sensibility and deeply subversive culture in the workplace, the cafe, the family, and politics. Cottereau's reconstitution of workers' practices from a fundamentally hostile text is fascinating and fruitful; he reveals "sublimism" to be not an archaic bit of folklore but a response to changes in and out of the shop. (To a poststructuralist, of course, Cottereau's interpretation, against his intentions, seems to endow Poulot's text with a single [inverse] truth. Rancière raised this point in a "conversation" with Cottereau published in *Les Révoltes logiques* in 1980.)

Faure, Dalotel, and Freiermuth unearthed a report chronicling almost one thousand public meetings held in Paris between 1868 and 1870. This document provides a record of the hitherto largely unexplored origins of the Paris Commune. The authors set out to show that these meetings were a popular phenomenon in which an increasingly radicalized and vocal populace asserted itself.

Proudhonian socialists and "Communists" were the most popular speakers. They shared hostility to the Second Republic and the Second Empire and support of the "social commune" but prefigured the Paris Commune in their disagreements over collectivization of property and the role of the state. (In an untranslated essay, the authors suggest that participants in the public meetings constituted a "workers' party" with an ideology akin to Marxism.) Dalotel and Freiermuth devote particular attention to pre-election meetings, which attracted larger, less radical audiences. Through analyses of popular responses to speakers, they discern a radicalization of meeting goers between 1869 and 1870. (This was not reflected in the elections, in which repression and political expediency dictated more moderate choices.) In 1869–70, audiences outdistanced even the radical speakers, putting their calls to violence into practice in street demonstrations.

The dominant theme in this excellent collection is the often-subversive relationship of the popular classes to authority—employers, the state, working-class republicans, and socialists on the stump. Individual songsters challenged the divide between manual and mental labor. The audience endowed July Monarchy theatrical productions and Second Empire political meetings with meanings. Male workers opposed the efforts of employers and republican militants to break down their familial bastion and to moralize popular culture. These essays deftly integrate insights derived from Michel

Foucault, French Maoism, and poststructuralism into social history. They are truly pathbreaking and deserve to be widely read; their questioning of concepts of workers' culture, urban social relations, and popular politics engage directly in what William Sewell, Jr., has recently termed in these pages "the (as yet insufficiently recognized) crisis" of labor history.

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CHRISTOPHE CHARLE. *Les Elites de la république, 1800–1900*. (L'Espace du politique.) Paris: Fayard. 1987. Pp. 556. 190 fr.

The charge is often made that social history turns away from politics. Tripping over useless data gathered for their own sake, social historians allegedly carry on a discussion of questionable value. In Christophe Charle's latest work on elites in *fin-de-siècle* France, all the trappings of social history appear, in particular, precise quantitative data about social origins, schooling, marriage patterns, wealth (or lack thereof), and committees and commissions on which intellectuals, bureaucrats, and capitalists served. This dense study, filled with both individual life stories on the one hand and statistical charts on the other, leads the reader to an opposite conclusion: social history is probably not apolitical but, rather, too political.

Charle examines the workings of the bourgeoisie as its members struggled for power and place in the Third Republic. He employs Pierre Bourdieu's schema of financial, social, and political capital to good effect in describing the "trumps" of various kinds that members of the elite employed to win new position or to gain a policy victory. The elite structure in France was such that an individual could move among its various sectors—industry, the high reaches of the bureaucracy, and the intellectual world. Such mobility, along with the tensions resulting from the contests for place, kept the elite structure vital. The ruling class always felt pressure and received revitalization from dominant groups. Only intellectuals had a difficult time negotiating the system. Self-appointed champions of pure intellect, they found themselves hard-pressed to maintain their intellectual capital in the "shuffling" of elites. In contrast, the *haut fonctionnaire* found himself in the best position for readjusting and reworking his capital.

Charle's conclusion, which reaches into the present, challenges current theses about the stagnation of French elites and about the apparent bureaucratization of everything from government to daily life. Charle admits that the French parliamentary system has weakened in the face of bureaucratization and planning. At the same time, bureaucratization has allowed as yet unmeasured amounts of shuffling among elites and the playing out of various kinds of capital. His evidence comes from studying patterns of membership on governmental commissions—patterns that reveal mem-

bers' skilled deployment of capital and that indicate continued fluidity among sectors of the elite.

For all the interest of those insights, Charle's study is set firmly in the Third Republic. This book provides an example of how the methods of social history have transformed the study of politics. Charle, like other social historians, shows that the play of power occurs in the reaches of social experience.

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PHYLLIS STOCK-MORTON. *Moral Education for a Secular Society: The Development of Morale Laïque in Nineteenth-Century France*. Albany: State University of New York Press. 1988. Pp. 231.

As Phyllis Stock-Morton reminds us in this nuanced and thoughtful work, French intellectual life in the nineteenth century was marked by a distinctive effort to create and to implement a *morale laïque*, a secular moral code for a postrevolutionary society. Stock-Morton finds in the period from Victor Cousin to Emile Durkheim an evolution in French moral thought from individualist philosophy to sociology, which parallels my own recent conclusions about the development of French liberalism in the same period (*From Philosophy to Sociology: The Evolution of French Liberalism* [1983]).

Building on the secular traditions of the Enlightenment, Cousin developed a purely secular moral philosophy aimed at the small minority of males receiving a secondary education in the early nineteenth century. At the same time he sought to remain on good terms with the church. Not until after 1848 did the secularizing movement openly break with the church, taking an anticlerical and even, with the Proudhonians of the *Morale indépendante*, an antireligious tone. All these efforts at a *morale laïque* suffered, the author thinks, from an excess of individualism.

The transition between this individualism and the "true social morality" of the turn of the century is found in Charles Renouvier. Stock-Morton gives a clear account of Renouvier's moral thought but underestimates both his break with Cousin and his modification of the Kantian tradition. She rightly shows that Renouvier's socializing of morality was as far in that direction as the French educational system was prepared to go during the Third Republic. Although Durkheim had a greater personal influence in the university, his moral philosophy had little impact on the vast majority of textbooks used in the primary and secondary schools.

Durkheim is shown to have been by no means the most radical of those who sought to make *morale* a science and, in particular, a social science, although Stock-Morton leaves us in some doubt about whether she agrees with those who considered him more of a philosopher than a "scientist." The failure of Durkheim to have a lasting influence on moral education is mainly attributed to the persistent individualism

of French society. On this difficult question of the relations between moral education and the social order, Stock-Morton takes a well-balanced position avoiding the popular reductionism that sees the schools as merely one of the tools by which a manipulative ruling class defends its privileges.

One of the particular strengths of this work is its combination of an intellectual examination of the evolution of moral philosophy with a history of moral education in the schools. Although this sometimes leaves us wanting to know more about many of the individuals or developments that are touched on, readers also profit from the author's attention to the larger picture. The major weakness of this work is a somewhat uneven effort to bring what appears to be an older manuscript up to date.

Finally, the State University of New York Press deserves a rap on the knuckles for a physical production unworthy of this fine scholarly work: errors range from missing endnotes to an inappropriate cover illustration to a style of binding more suitable for a secondary textbook.

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JEAN-YVES MOLLIER. *L'Argent et les lettres: Histoire du capitalisme d'édition, 1880-1920*. Paris: Fayard. 1988. Pp. 549. 180 fr.

In his meticulous case study *Michel et Calmann Lévy ou la naissance de l'édition moderne 1836-1891* (1984), Jean-Yves Mollier called for a comparative history of publishing. His own response to that call, *L'Argent et les lettres*, represents business history at its best. A painstaking examination of commercial, financial, and legal records lays the solid foundation for a reconsideration of French economic history as well as French literary culture. Along the way we learn a great deal about the often-fascinating individuals who made the world of French publishing what it was and, in important respects, still is.

Mollier constructs his comparative framework from detailed case studies of Parisian publishers chosen to represent different sectors of publishing and different types of publishers. Although it looks back to the printers of the *ancien régime* and forward to the multinational conglomerates of today, the study centers on the crucial transition from the entrepreneurs who dominated publishing at the beginning of the nineteenth century to the large-scale publishing enterprises that emerged at the end—the transformation from Michel and Calmann Lévy to Calmann-Lévy, from Louis Hachette to the giant Hachette et C^{ie}. The pioneers operated small-scale, traditional businesses; their successors, to be successful, had to transform those enterprises into exemplars of full-blown industrial capitalism. Those who did not fully accept the implacable logic of corporate capitalism, who forgot the lessons of the founders and rested on their laurels

(and their backlists) instead of actively seeking new markets, could expect their houses to fail or stagnate.

Mollier's book makes as significant a contribution to literary history as it does to economic history. It calls into question a good many *idées reçues* about individual publishers and publishing and about particular authors and French literary culture. Cutthroat practices that have been denounced as the by-products of recent corporate mergers also turn up over a century ago. Competition today can scarcely be fiercer than it was during *le krach* of the 1890s, when overproduction prompted publishers to seek capital return outside publishing (real estate was a traditional favorite). The mergers that resulted, such as Charpentier-Fasquelle, left the field open for aggressive newcomers such as Gaston Gallimard and Bernard Grasset, who started the cycle anew.

The legend of the publisher-patron also falls in these pages, which detail contract after contract drawn up by publishers intent on driving a hard bargain. Then as now, few writers offered more than token resistance to the conditions offered, so eager were they to get into print. Rationalization early on led to conflict, and by 1890 Calmann-Lévy had established a legal department. Mollier disputes a number of current interpretations, notably Pierre Bourdieu's emphasis on the preponderant role of symbolic capital, major as well as minor claims in volume 4 of Roger Chartier and Henri-Jean Martin's *Histoire de l'édition française* (1986), and the embellishments of what turn out to be self-serving memoirs, naive biographies, and erroneous accounts by contemporaries.

Mollier bases his often severe but always scrupulous judgments on an impressive number and range of sources: balance sheets, contracts with authors, marriage contracts, tax declarations, notarial archives, declarations of bankruptcy, acts of incorporation, post-mortem inventories, correspondence, and even police reports (documenting, for example, the debut of the Garnier brothers peddling pornography). Beyond business history, economic history, and literary history, Mollier's readings of those documents reconstruct as well the drama of social mobility, the intense investment in matrimonial strategies, the pride in success, the distress of inheritance (the Larousse debacle is worthy of soap opera), the friction between generations (Gervais Charpentier all but disowned his son Georges), the pathos of failure (Georges Charpentier), and the brutality of politics (the "aryanization" of Calmann-Lévy during the German occupation).

In sum, this book is an outstanding work. It is indispensable for the history of publishing, vital for a full appreciation of writers' positions in the literary field, and important for a better understanding of nineteenth-century French society. Fayard is to be congratulated for bringing out this important work at a reasonable price.

PRISCILLA PARKHURST FERGUSON
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WILLIAM BRUSTEIN. *The Social Origins of Political Regionalism: France, 1849–1981*. (California Series on Social Choice and Political Economy, number 17.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xiv, 243. \$28.00.

William Brustein's study advances the argument that political regionalism follows modes of agricultural production. He rejects normative and structural explanations of consistent patterns of regional voting, for example, perpetuation of electoral conservatism in western France as a result of durable social structures and relations, the "transmission of values and norms from one generation to the next as the principal determinant of political preferences" (p. 9), or the collective memory of revolt of the *chouans* against the revolution.

Brustein presents a survey of three regional economies before and after 1865—a date he selected because of the completion of the Paris-Brest railroad line—and he contrasts them: a high degree of commercial activity and resultant close ties between city and country, nucleated villages, medium- and small-sized farms, and cash tenancy of the "northeastern" mode, including Upper Normandy, Flanders, Picardy, and the Ile-de-France, characterized by increased large-scale industrialization and an agricultural revolution after 1865; in the west, subsistence farming and sharecropping, dispersed settlement (the famous *bocage*), hostility between city and country, extreme class differences, and strong church influence; and, in the Mediterranean region, the limited and even decreasing role of industry, close ties between town and country, small farms, relatively weak church influence, and relatively democratic class structure, conducive to the flowering of institutions of popular sociability.

Brustein seeks to demonstrate that, at least in the crucial elections of 1849, 1914, and 1981, voters in these three regions made their political choices in accordance with their economic interests, largely determined by the dominant mode of production. Taking measures of economic activity, property rights, patterns of settlement, class composition, tradition (a canton's previous vote for the Left), and religiosity (proportion of adults regularly at mass on Sunday), he shows, in contrast with other interpretations, that small tenant farmers and sharecroppers voted for the parties of the Left, supporting plans for land distribution and progressive taxation.

Yet Brustein's thesis in some ways is as simplistic as those he has intelligently criticized. With Le Roy Ladurie's distinction between parachutists and truffle hunters in mind, Brustein has undertaken some serious parachuting. So much changed between the elections of 1849 and those of 1981 that one wonders how much faith to place in this sort of inquiry. The area may be too large a unit of analysis, given the great variations found within each region.

Brustein's satellite view of the French countryside catches some important general differences between

regions but inevitably misses much. For example, in noting that the "better-off zones of Béziers and Narbonne became more supportive of the socialist movements than those of Montpellier and Nîmes" (p. 23), he neglects the persistence of popular allegiance to the Catholic church in the *pays* of Protestant violence against Catholics during the Revolution, paid back *avec usure* during the White Terror. Here the mode of production theory is of little help, and the collective memory interpretation, however old-fashioned, is probably more satisfying. Historians of religion and those of the West are not likely to be satisfied with Brustein's interpretation for the persistence of religiosity: "In western France the mode of production has created an environment in which the Catholic church serves as the principal provider of resources (that is, of employment, charity, education, land, and social activities)" (p. 156).

Finally, if one were to carry Brustein's analysis beyond the election of 1981, the mode of production explanation would fail to catch the remarkable shift of the Mediterranean littoral from the Left toward the Right (although he does indicate traditions of political conservatism in some Mediterranean cities dominated by prosperous landowners in the nineteenth century). Jean-Marie Le Pen's success in recent national elections was most marked in departments such as the Var, Aude, Hérault, Pyrénées-Orientales, and Gard as well as the Alpes-Maritimes. There Le Pen made a stronger showing than in departments of traditional conservatism, such as his Morbihan. These departments swung toward the (far) Right because of two factors: the presence of large numbers of North African immigrants, who obviously did not vote for Le Pen, and the presence of many former French *colons* who left Morocco, Tunisia, and, above all, Algeria following the independence of those former colonies.

There are other problems as well. Brustein's penchant for repetition weighs on the reader (such that virtually entire sentences are duplicated, for example, page 146). References to the "Debre law" (p. 114) and to the socialist leader "Jean Juarès" (p. 217) inevitably reduce confidence. This is particularly unfortunate because many readers will need to be convinced, in view of the global nature of the argument and its application over almost one hundred fifty years of history in three complex "regions," that Brustein knows France. Yet this bold study deserves to be read. Its conclusion offers a survey of rural voting patterns in other countries, ranging from the Populist movement in the United States to Spain during the Civil War, Nazi Germany, and India in the 1960s. If the book provokes comparative thinking, it will have succeeded.

JOHN MERRIMAN
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RENE REMOND and JEAN-FRANÇOIS SIRINELLI. *Notre siècle, 1918–1988*. (Histoire de France, number 6.) Paris: Fayard. 1988. Pp. 1012. 198 fr.

This masterful study analyzes the traumas and enormous changes that France has undergone in the past seventy years: two world wars; the German occupation ("a disaster whose suddenness and size took French memories back five centuries to the time of the Hundred Years' War" [p. 941]); colonial wars (between 1914 and 1962 France was at war twenty-six out of forty-eight years); two major economic crises; four political systems (one of which suppressed democracy); recurring threats of civil war; economic modernization; and social and cultural transformation on an unprecedented scale. René Rémond makes indelible the period's tragic side ("the hundreds of thousands of young Frenchmen who fell at Verdun or on the plains of Champagne, in the rice paddies of Tonkin . . . , in deportation camps or before firing squads" [p. 942]), but he also hails its triumphs, especially the innovation and reform that has marked the last forty years, on the Right as well as on the Left. As Rémond notes, "France went between the 40s and the 60s from the deepest abyss and absolute misfortune to the heights of prosperity" (p. 943).

Although, even during the 1930s, France "was not completely a blocked society" (p. 944), it changed from a country stuck in routine, paralyzed by tradition, and opposed to change to a nation that first accepted and then honored innovation. Pierre Mendès France, Edgar Faure, Charles de Gaulle, Valéry Giscard d'Estaing, and Laurent Fabius were all modernizers of various kinds. Indeed, according to Rémond, the Right, whose reference point was once the past, has proved more innovative than the Left. A new "religion of change" has affected all sectors of French life. French farmers in the last thirty years have increased production eight-fold with new techniques, while France has gone from mainly a rural population in 1918 to an overwhelmingly urban population today. Nothing has changed more than the role of women in society, "probably the most important fact of the last half-century" (p. 951). More than nine million women work alongside twelve million men, and "one can calculate the time that they will be as numerous" (p. 951).

There have also been fundamental cultural changes, especially those resulting from the enormous impact of television. Some of the most interesting chapters in this book are written by Rémond's collaborator, Jean-François Sirinelli, who treats the major cultural, artistic, and intellectual developments of the period, from the "roaring twenties," when the new "modernism" in the arts was supposedly king (but, in fact, was not), to the "yé-yé" generation of the 1950s and the new mass culture of the 1980s, when Anglicized adolescents, with money to spare, caused older intellectuals to question whether "culture" in any serious sense would survive.

Indeed, Rémond worries in a pensive concluding chapter whether the very existence of a French identity has not been fundamentally undermined by all of the changes that have taken place since 1945, especially by the internationalization of culture, the destabilization

of the family through divorce and working mothers ("it has always been through women that societies have endured and the values of civilization have been transmitted" [p. 950]), and by other departures from tradition. For Rémond, unlike Erik Erikson, sees identities as rooted more in a sense of the historical past than in a sense of present or future callings, and his book is written in part in the service of such an identity.

The great strength of this work stems from Rémond's gift for comparing historical events, often separated by fifty years or more, in a way that reveals their true import. Thus, he notes that, although French rightists caused a terrible row over the Stavisky scandal of 1934 (which sparked the riots of February 6 that overturned a left-center government representative of the electorate), the Stavisky scandal was a minor affair compared to the Panama scandal of an earlier era. The same perspective, however, leads Rémond to devalue the importance of French fascism in the 1930s on the grounds that the rightist leagues of the period, including the Croix de Feu, were more traditionally conservative than fascist. Yet both Italian and German fascism had deep roots in their nations' histories—and were no less brutal for it.

This book is a brilliant, subtle, and commanding survey of one of the most resilient societies of modern times, written by two of its most thoughtful historians.

ROBERT SOUCY
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R. C. GROGIN. *The Bergsonian Controversy in France, 1900–1914*. Calgary: University of Calgary Press. 1988. Pp. x, 222. \$19.95.

In this thoroughly researched and carefully argued monograph, R. C. Grogin makes a good case for his claim that, between 1907 and 1914, Henri Bergson (1859–1941) was the world's most controversial philosopher and an international celebrity, probably the first of the intellectual media stars who have become more common figures in the later twentieth century. Grogin helps us understand why, as recently as 1987, the "new philosopher" Bernard-Henri Lévy, himself a television personality and perhaps an unwitting and unwilling heir to Bergson, could denounce the "diffuse Bergsonism" that in his view has been one of the major sources of modern French philosophy.

Grogin also documents the tremendous popularity Bergson enjoyed in the United States. Although in his early career Bergson tended to be a withdrawn, apolitical intellectual and did not take an active position during the Dreyfus Affair, he became ardently patriotic and fully *engagé* during World War I. Bergson undertook two missions to the United States, meeting privately with President Wilson. There is some evidence that Bergson played a role in influencing Wilson's decision to call for a declaration of war against the Central Powers in April 1917.

Grogin is completely familiar with Bergson's biogra-

phy and his philosophy and is a skilled expositor of his doctrines, how they evolved, and the principal influences on his thought, including the occult revival in *fin-de-siècle* France. Grogin succeeds admirably in elucidating Bergson's rather vague, even mystical concept of *élan vital*, the heart of his most famous and influential work, *Evolution créatrice*, published in 1907.

Grogin devotes several astute pages to analyzing Julien Benda's extraordinarily passionate and unremitting attack on Bergson and Bergsonism, which began in 1912 and only ended with Benda's death in 1956. Grogin is able to explain why Benda, who is best remembered as an advocate of the ivory tower in *La Trahison des clercs* (1927), could state that he would have killed Henri Bergson for purely ideological reasons.

But, when Grogin attempts to move from the outside in, that is, to meet his stated goal of solving the "Bergson problem," delineating the factors in French and European prewar society that made Bergson so amazingly popular, he is less successful. Grogin could have examined in more detail the right-wing implications of Bergson's thought and explored potential linkages to the nationalist revival in France, which peaked just prior to the outbreak of World War I. There have been important works published in recent years, which Grogin does not cite, that illuminate the social and cultural context that made the shy, elusive, and spiritual Bergson so notorious. For example, Charles Rearick's *Pleasures of the Belle Epoque* (1985) offers fascinating insights into the complex cultural mood that required constant new entertainment and festivity to stave off the sense of "sitting on a volcano."

Although he has admirably synthesized the principal features of Bergson's philosophy and conclusively demonstrated the reality of Bergson's fame, Grogin does not offer a satisfying explanation of why Bergson's disciples made pilgrimages to his summer home in Switzerland, collecting locks of hair from his barber-shop as religious objects.

DAVID L. SCHALK
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JEAN-FRANÇOIS SIRINELLI. *Génération intellectuelle: Khâgneux et normaliens dans l'entre-deux guerres*. Paris: Fayard. 1988. Pp. 721. 250 fr.

This collective biography of a relatively small number of intellectuals illuminates much of the French political landscape from the 1920s to 1945 and beyond. Jean-François Sirinelli's subjects are two related groups: the literary students who entered the Ecole Normale Supérieure in the years from 1920 to 1931 and the candidates (*khâgneux*) who prepared for the school in special classes in Paris, Lyon, and several other provincial capitals. In the nineteenth century only occasionally did *normaliens* exercise any influence beyond academic and literary spheres, but, after the Dreyfus Affair, many of them found positions in journalism, government administration, and politics, and thus they

became more visible among their contemporaries and more intriguing to historians.

Central to Sirinelli's study is the concept of a "generation," in this case students old enough to have been marked by World War I but too young to have fought in it. This "generation of 1905" (most born between 1900 and 1910) shared much with previous generations of *normaliens*, but their unique historical experience set them apart and made them particularly receptive to socialist and pacifist ideas.

The author has resisted, however, any temptation to reduce his subjects to a set of reified causes. Indeed, the book is, in large part, a vast collection of short biographies that show the intersection of individual "trajectories" in "microclimates" of shared sensibilities within a broader historical context. Typically from staunchly republican families, these *boursiers conquérants* made the Ecole Normale Supérieure the nerve center of left-wing politics within the more conservative Latin Quarter. But pacifist ideas and agitation, Sirinelli maintains, were most notable at the rue d'Ulm in this decade and gave the students' socialist-radical consensus both its points of cohesion and its fragility. Emile Chartier ("Alain") did indeed have an influence at the Ecole Normale far beyond the mere handful of students who had actually sat in his class at Henri IV. But ultimately these disciples, faced by political realities of the 1930s and 1940s, had to decide whether to renounce the pacifist idealism of their formative years. Most did so and subsequently supported Charles de Gaulle or the Resistance, but several held firm and incurred suspicion or prosecution after the war for their refusal to choose sides. Only a few wholeheartedly supported either the invader or the Vichy regime.

This study resembles the recent work of John F. Sweets, *Choices in Vichy France: The French under Nazi Occupation* (1986), in that it explains convincingly and in great detail the comportment of a distinct group during the Vichy years. Whereas Sweets concentrated on citizens of a geographical area during the Vichy period itself, Sirinelli has shown how the alumni of a *grande école* evolved politically over about twenty years. Both conclude that real collaboration with the enemy or even, in the later stages, with the Vichy regime itself was exceptional. Those among Sirinelli's protagonists who delayed or refused to oppose Vichy did so, typically, not out of sympathy for an authoritarian regime but because of a moral imperative to refuse to take up arms, even in response to unspeakable provocation.

In addition to taking full advantage of traditional sources, the author has generated new ones. In 1976, perhaps noting that by then his protagonists were eager to offer their version of the past, Sirinelli sent them a questionnaire, which elicited long written responses and led to further correspondence and interviews. But he has used this material with superb caution, frequently leading the reader through a laborious process of checking sources against each other to shed new light on well-known figures such as Raymond Aron, Paul Nizan, Jean-Paul Sartre, and Simone Weil

as well as others such as Georges Lefranc and Guillaume Guin  y. In short, one can also read this remarkable study as a primer in the historian's craft.

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JEAN-PIERRE RIOUX. *The Fourth Republic, 1944–1958*. (Cambridge History of Modern France, number 7.) Translated by GODFREY ROGERS. New York: Cambridge University Press. 1987. Pp. xv, 531. \$59.50.

It is difficult to praise enthusiastically enough this history, first published in France in two volumes in the series *Nouvelle histoire de la France contemporaine* (1980, 1983), of Charles de Gaulle's provisional regime of 1944–46 and of the Fourth Republic. Even though Jean-Pierre Rioux did not have at his disposal the archives and private papers future historians will be able to use, I doubt that his judgments will be found wrong or unfair.

The best thing about this book is the mix of thoughtful moderation and sharp evaluation. Rioux does not conceal the multiple failings, of both the French as a nation in those turbulent years and the political system of the Fourth Republic. For instance, he is very critical of the postwar purges, which often amounted to a search for scapegoats instead of a self-examination that would have challenged the French view of themselves as victims or resisters. But he is never a prosecutor, and he points out how much vitality the French displayed after the dreadful ordeal of decline in the 1930s, collapse in 1940, Vichy degradation, and Nazi occupation. The chapters he devotes to economic modernization—France's demand-led growth, a spectacular boom driven by a thirst for consumer goods—are well documented and comprehensive. Rioux gives due credit to the tenacious technocratic civil servants who guided France's economic overhaul behind a facade of short and weak governments. As for the institutions of the Fourth Republic, Rioux puts the blame for the regime's flaws less on the constitution than on the state of the main parties and on the mediocrity of many of their leaders.

Rioux's narrative talent carries the reader through the maze of internal crises, colonial debacles, foreign policy initiatives and contradictions, intellectual debates, and social transformations that marked one of the fullest periods of French history. The French emerged from the drama of defeat and resistance with a will to modernize, an ardent desire for a more just society and a more democratic but also more effective political system, and the expectation of a restoration of their power position in the world. Some of those ambitions proved beyond their reach; those that were achieved (such as modernization) created problems of their own (inequality in the distribution of goods and the threat of mass culture). Abroad, the real imperative was adaptation to a world dominated by superpowers

outside Europe and by the drive of colonized peoples for self-determination; restoration and adaptation were incompatible, especially in a country whose political class was racked both by the political cleavage between communists and non-communists, created by the cold war, and by the cleavage between colonialists and liberals, created by decolonization. Those two intersecting lines of division made coalitions precarious and allowed for only one kind of lasting majority: a paralyzed one. How the French coped with those issues, how political mediocrity went hand in hand with intellectual and artistic brilliance, how the pull of traditional values and of past grandeur in Europe and in the empire conflicted with the powerful call of rejuvenation, all that is told by Rioux with clarity, elegance, and an empathy that never obliterates critical judgment.

Rioux is one of the ablest members of the very brilliant school of contemporary historians who work at the Institut d'histoire du temps pr  sent—men and women who may be less flashy or celebrated than the members of the *Annales* but who are certainly just as competent and readable and who are far less oblivious of the importance of political history than many of the *Annalistes* have been. For students of contemporary France, Rioux's book is compulsory reading. Godfrey Rogers's translation is satisfactory.

STANLEY HOFFMANN
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MAURICE LARKIN. *France since the Popular Front: Government and People, 1936–1986*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xix, 435. Cloth \$64.00, paper \$19.95.

This book is a welcome addition to the literature on contemporary France. It will inevitably find its way onto graduate reading lists and may well be adopted as a text in undergraduate courses in French history, but it may also be read with profit by the specialist. Maurice Larkin has accomplished a kind of tour de force; there is no equivalent work in English or French that covers the history of France during the last fifty years with such depth and originality. Among this book's strengths are its comparative perspective and its treatment of French economic development. It is extremely useful to have the most important statistical data on French economic growth brought together for easy reference, and the material is much more enlightening when continuously juxtaposed to relevant data from France's European partners. Another highlight is Larkin's attention to demography and related social issues. The birth rate is of importance. The postwar baby boom, for example, explains in part the crisis of May 1968. It also provides the impetus for Larkin's discussion of social issues. It is interesting to know that as many as one-third of French pregnancies ended in abortion during the 1930s, which demonstrates the futility of legislation designed to encourage fertility by

outlawing both abortion and the sale of contraceptives. Larkin's discussion of French attitudes is equally perceptive: anyone who has cashed a check in France prior to the advent of *guichets automatiques* will recognize and enjoy Larkin's description of the process.

Politically Larkin organizes his narrative around the "three Cs" (pp. 36, 163), a convenient shorthand that emphasizes the attenuation of the traditional divisiveness of the clerical, constitutional, and class issues that have agitated the French body politic from Léon Blum to François Mitterrand. We may add a fourth C for colonial questions. Larkin appears to regard clerical, constitutional, and colonial issues as more or less settled; the multiparty division they once entailed has been simplified into the polarization of Right and Left around class that characterizes French political life today. Yet the question of the government versus the Catholic schools gave rise to the largest demonstration in French history against Mitterrand in June 1984, and the New Caledonia issue appeared to monopolize the French headlines in 1988–89, making one wonder whether Larkin's beguiling picture of emerging consensus on the four Cs is not overdrawn.

Scholars will quibble with other of the author's rather summary judgments. I doubt that, had Philippe Pétain fled to North Africa in November 1942, he would have emerged later as the hero of the Liberation. The rot of Vichy had spread too far by then. It is also grossly inadequate to say that only 1 percent of the French participated in the Resistance, as John Sweets has recently shown. Paul Ramadier had no need of American prodding to rid the government of Communist participation in May 1947. I doubt that evidence could be found to substantiate the claim that Washington subsidized the Gaullists after 1947; U.S. payments to *Le Populaire* and to the moderate union, Force Ouvrière, were quite enough. Was Charles de Gaulle's cry of *Algérie française* really ambiguous in 1958, or did the general simply change his mind later as he became convinced that the war was unwinnable? Larkin does well to emphasize, however, that de Gaulle's foreign policy was carefully designed to build a national consensus so as to make palatable his very conservative social policies at home; the tremendous growth rates achieved during the unprecedented boom from 1947 to 1973 were not shared in measured proportion with the working class. That fact and the inadequate university structure explain the eruption of the alliance of students and workers that eventually drove de Gaulle from power.

May 1968 forced a genuine sharing of the benefits of the new prosperity, which had to be accomplished amid relative recession after 1973. Larkin is right to call our attention to the reformist urge that characterized Giscard d'Estaing's "Orleanist" presidency, but the picture is somewhat overdrawn. Similarly, given the vast changes that took place under Mitterrand, it is an exaggeration to insist on the conservative, technocratic nature of Socialist rule under the ministry of Laurent Fabius after 1984. France is still absorbing the conse-

quences of the minirevolution of 1981–83. One may further fault Larkin for all too hastily sketching the momentous rebirth of the Left under Mitterrand in the 1970s, the signing and rupture of the Common Program, and the corresponding decline of the French Communists. His treatment of French intellectual life during the twentieth century could also have been a bit more thorough without adding too much to the length of the book. Those caveats aside, this book will be much appreciated by specialists and generalists alike.

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MARVIN LUNENFELD. *Keepers of the City: The Corregidores of Isabella I of Castile (1474–1504)*. (Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1987. Pp. xi, 290.

Isabella of Castile has enjoyed the rare good fortune among politicians of having posterity accept her own view of her accomplishments. That her reign witnessed the successful assertion of monarchical authority over a fractious aristocracy and other privileged orders is one of the more durable myths of Spanish history. The underlying theme of this fine book is an attempt to call into question the traditional view of Isabeline law and order by focusing on the *corregidores* appointed by the crown to oversee the leading cities of the kingdom.

Although not the first monarch to vest substantial administrative and judicial powers and responsibilities in the hands of those officials, the Catholic queen relied on them to an unprecedented degree to affirm the local presence of the crown. The results of that initiative were mixed. During the early years of her reign, the *corregidores* did indeed contribute to the pacification of Castile by promoting badly needed reforms in civic administration. Of particular importance was the role they played in negotiating between rival factions (usually of noble origin) in municipal government, which habitually resulted in a new division of offices that strengthened local oligarchies to the detriment of popular representation. The resumption in the 1480s of war against the Muslim kingdom of Granada, however, led to a dramatic increase in the fiscal needs of the crown. The military and financial support of the church, aristocracy, and crusading orders was bought by relaxation of monarchical pressure on their privileges and exemptions. The cities, and especially urban commoners, paid the price of that social contract. Meanwhile, the *corregidores* found themselves caught in the middle, weighted down with a bewildering array of obligations yet not provided by the crown with the backing needed to discharge their responsibilities. Small wonder, then, that shortly thereafter both urban elites and lower classes joined ranks in the *Comunero* revolt in a vain attempt to force the crown to carry out the ambitious program proclaimed, yet not achieved, during Isabella's early years in office.

In one of his last and best-known essays, "The Administrative Structure of the State in the Sixteenth and Seventeenth Centuries" (Henry J. Cohn, ed., *Government in Reformation Europe, 1520-1560* [1971]: 60-61), Jaime Vicens Vives cautioned historians of public institutions not to ignore the "tumultuous reality" underlying the "simple practice of government." Marvin Lunenfeld has taken this admonition to heart. Although earlier studies of the *corregidores* were based almost exclusively on examination of royal decrees and instructions—hardly surprising given the overwhelming influence of legal historians on the political and administrative history of early modern Spain—Lunenfeld has drawn on a much wider range of sources. The result of his research, which centers on the abundant correspondence between the monarch and local officials (the correspondence is housed in the Simancas archive), is an extremely detailed chronology that firmly inserts government offices and officeholders within the context of local politics and society. An appendix (pp. 196-226) listing the postings and academic credentials of over four hundred of Isabella's appointees concludes this significant contribution to our knowledge of a critical, yet largely unexplored, period in Spanish history.

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COLIN MARTIN and GEOFFREY PARKER. *The Spanish Armada*. New York: W. W. Norton. 1988. Pp. 296. \$27.50.

FELIPE FERNANDEZ-ARMESTO. *The Spanish Armada: The Experience of War in 1588*. New York: Oxford University Press. 1988. Pp. x, 300. \$24.95.

PETER PADFIELD. *Armada: A Celebration of the Four Hundredth Anniversary of the Defeat of the Spanish Armada, 1588-1988*. Annapolis, Md.: Naval Institute Press. 1988. Pp. 208. \$24.95.

The four hundredth anniversary of the defeat of the Spanish Armada in 1588 has occasioned at least six new books on the subject, two catalogues for the splendid Armada exhibit at the National Maritime Museum, Greenwich, and a reissue of Garrett Mattingly's Pulitzer Prize-winning *Armada* (1959).

The three books reviewed here and *Armada*, the official catalogue of the Armada exhibit, edited by M. J. Rodriguez-Salgado (1988), are the best of the new crop, and each has special merits. Although none of the three surpasses Mattingly's book in diplomatic scope and pure drama, all treat the campaign better and take advantage in varying degrees of recent research, in which underwater archaeology has played a part. Since 1959 our understanding of sixteenth-century ships, shipbuilding, gunnery, and navigation has increased considerably. All three authors also use I. A. A. Thompson's "Spanish Armada Guns" (*Mariner's Mirror* 61 [1975]: 335-371), which convincingly reasserts what contemporaries believed, namely, that the English had

bigger and better guns, and which revises the flawed work of Michael Lewis, on which Mattingly depended, that credited the Spaniards with the greater weight of shot.

Colin Martin and Geoffrey Parker have produced the best account of the Armada campaign yet written. It rests on new archival data, fresh treatment of material long available, Martin's work on underwater archaeology (see his study of Armada wrecks, *Full Fathom Five* [1975]), and thorough knowledge of the literature, contemporary and modern. The authors have crafted the book to achieve something of Mattingly's dramatic sweep while offering keen analyses of each phase of the Enterprise of England, from its inception to its aftermath, and a wealth of technical detail.

Martin and Parker begin with a survey of the two fleets as the Armada came in sight of England. The Armada's task was to cover the landing on the Kentish coast of the duke of Parma's Army of Flanders. The Spanish fleet's tactics were largely defensive, and sea fights were primarily the business of some twenty principal ships that the authors dub "trouble-shooters." They sensibly observe that the squadrons appearing on the printed roster did not fight as discrete units; rather those vessels were deployed throughout the Armada. The task of the well-described English fleet was "to impeach it" (p. 47).

With the fleets poised for battle, the authors take up in masterly fashion the background of the war, drawing on Parker's previous work on the Dutch Revolt and pursuing the souring of relations between Philip II and Elizabeth, in which they see the Ridolfi plot of 1571 as the turning point and Sir Francis Drake's raid on Vigo in 1585 as the last straw that committed Philip to invasion. The authors' handling of the evolution of strategic planning for the enterprise must now become the standard account. They conclude that the decision to unite the Armada and the Army of Flanders prior to the invasion proved fatal (p. 118). Their portrait of the architect of the grand design, Philip II, is truer if less vivid than Mattingly's; it is the king we met in Parker's splendid *Philip II* (1978), who somehow fell ill at moments of crisis and thus slowed the decision-making process.

The preparation of the Armada and the selection of the duke of Medina Sidonia to succeed the marquis of Santa Cruz in its command receive due attention before the authors return to the confrontation in the Channel. Their discussion of the ensuing battles as the Armada advanced to its rendezvous with Parma rests securely on their research. They catch well the suspense with which Parma awaited news of the Armada before he dared embark his troops in face of the Dutch blockade. His first solid news was followed two days later by the Armada's arrival at Calais; he had barely issued the embarkation orders when he learned that the Armada had been forced to sea again by the English.

In an outstanding chapter, "Anatomy of Failure," Martin and Parker reveal that, from inventories of

returned Armada ships and evidence culled from sunken wrecks off the Irish and Scottish coasts, many Spaniards fired barely one shot per gun per day in the fighting. The authors ascribe this performance partly to Spanish tactical doctrine, which was not to fire big guns until almost close enough to board, and partly to clumsy two-wheeled gun carriages and shortage of trained gun crews able to reload under fire. The English with their more nimble ships would not allow the Spaniards to board, and, when the English discovered the ineffectiveness of Spanish gunnery, they closed the range. Off Gravelines, after forcing the Armada from the anchorage at Calais, the English gave the Armada a fearful drubbing, which would have been worse had they not run low on powder and shot and ended the engagement. The book contains well-selected illustrations, including most of the well-known Adams charts and Pine engravings, which successfully augment and enrich the text.

Felipe Fernández-Armesto admittedly has written a *pièce d'occasion* yet makes a nice contribution to the literature. He attempts to do what John Keegan did in *Face of Battle* (1976), that is, describe what the campaign was like for ordinary soldiers and seamen, though chiefly on the Spanish side for which there is more evidence. He writes with flair and establishes from the start the human dimension of his narrative with Miguel de Cervantes riding out from Seville to collect taxes for the Armada. Fernández-Armesto is the most vivid of our authors in treating religious differences in popular terms and the perceptions each side had of the enemy.

When he returns to high politics, planning, and the broader treatment of battles, his lack of familiarity with more than the standard material in print frequently shows, although he poses many astute questions and offers provocative analyses of the development of Spanish strategy and the conduct of the campaign. He has made good use of contemporary English and Spanish fighting instructions in his efforts to reconstruct the encounters in the Channel. But his treatment of details of the Armada's gunnery problems suggests an acquaintance with the public lectures that Geoffrey Parker has given on the topic for several years, which deserve specific citation. Fernández-Armesto seems unfamiliar with the work of J. F. Guilmartin, *Gunpowder and Galleys* (1973), regarding black powder ballistics and gun-barrel length as well as recent research regarding ships, ship types, and ship building.

Ships and gunnery have been two of Peter Padfield's lifelong interests, and he brings to his lively narrative the good sense and insights of one who knows the sea. In dealing with high politics, he draws selectively and usually well on the work of others. When he reaches his own areas of special competence, he seems sound about English ships, less so about the Spanish, although for everyone there remains much to learn. His treatment of guns and gunnery has much to commend it, and he knows the debate about Guilmartin's work. Padfield does for English crews and their seagoing life what Fernández-Armesto does for the Spaniards. He is not

inaccurate regarding the harsher conditions aboard the Armada, but his explanation of them does carry the taint of traditional English prejudice.

In his chapters on the campaign, Padfield offers a better narrative than does Fernández-Armesto and provides it with well-conceived battle charts that show wind and tides and an excellent map displaying pertinent weather information. He even includes a few things that escaped Martin and Parker, although he is less successful than they in integrating his illustrations with his narrative.

The volume by Martin and Parker belongs beside Mattingly's, and all three belong in the collections of college libraries and Armada buffs.

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DAVID C. GOODMAN. *Power and Penury: Government, Technology, and Science in Philip II's Spain*. New York: Cambridge University Press. 1988. Pp. xii, 275. \$44.50.

During the past decade, Cambridge University Press has been extraordinarily active in publishing work on early modern Spain. Now comes a book in which David C. Goodman examines an unfamiliar aspect of Philip II's realm: the limitations and achievements in the application of the natural sciences to problems of government during Philip's reign. The first of the book's five chapters is probably the least interesting. In it Goodman deals at length with Philip's attitude toward the "occult sciences," concluding that he viewed them with "restrained curiosity" (p. 9). The second chapter concerns cosmography, offering us a good survey of Philip's precocious cartographic ventures both in the peninsula and in the New World. Here, as elsewhere, Goodman draws on a wide range of little-used archival material to good effect. Curiously, though, he makes no mention of North American scholars such as Howard Cline and Ursula Lamb, whose contributions in this area have been important.

In the third chapter, we come to technology for war: shipbuilding, gun founding, production of munitions, and "secret inventions" (p. 129-41). Philip was undoubtedly open to new ideas in all of those areas, and the geographical extent of the empire allowed him easily to import the best foreign experts: miners from Germany, for instance, and engineers from Italy. If the results of Philip's effort were often disappointing, it was due as much to shortage of money as to technological inadequacy. Goodman makes those points convincingly, though here and there his arguments seem simplistic. He remarks, for instance, that the Spaniards and their enemies were roughly equal in technological competence, for "if the Dutch had possessed any advantage in military technology the struggle against Spain in the Netherlands would not have lasted eighty years" (p. 141). The duration of the struggle surely demands a more complex explanation than that.

Chapter 4 concerns the mining of silver and, in

particular, "the rise and fall of Guadalcanal" (p. 151–72). Goodman offers a fresh perspective on the fluctuations in Philip's supply of precious metals, taking into account production in both the Old World and the New World. As in the other chapters, the plates in this one are few but very well chosen. It is a pity, though, that we are never told or shown what today survives of Philip's ventures in mining and gun founding.

The final chapter is a fascinating and original survey of "the crown's interest in medicine" (pp. 209–60). Philip had much personal contact with doctors and gave much attention to the organization of medical services in his realm. The army's field hospitals, for instance, were exceptionally well organized and staffed, and scientists such as Francisco Hernández, who led an expedition to the New World in search of new medicinal plants, received wholehearted royal support. In general, Goodman's image of Philip II is rather like the one offered by Geoffrey Parker in his recent biography; here was a king who was keenly interested in the natural sciences but whose attempt to apply scientific solutions to administrative problems was hobbled by fiscal and racial problems.

The book has some omissions. There is very little about mathematics and its important military application in ballistics. Some works that seem of central importance are not cited, such as the *Apuntes para una Biblioteca Científica Española del Siglo XVI*, published by Felipe Picastoste y Rodríguez in 1891 but still a mine of information. The list of sources vexatiously contains only printed material, leaving us with no idea of the shape of Goodman's investigations in the archives. Still, this is a short work on a huge and neglected subject, and we should be grateful for the great quantity of new information that it does bring.

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MANUEL TUÑÓN DE LARA *et al.* *La guerra civil española: 50 años después*. 2d ed. Barcelona: Editorial Labor. 1986. Pp. 476.

This volume has already gone through several editions, understandably so. Seven essays by five authors provide a comprehensive review of the present scholarly consensus on the politics, foreign policy, economy, and culture in both the Republican and the Nationalist zones during the Spanish Civil War. Each includes a bibliography of all major works and documents, rigorously selected from among the thousands published in English, French, and German as well as Spanish over these past fifty years.

The authors of the four core articles reflect specifically the current reconceptualization of civil war phenomena taking place in a national and international ambience that is in no obvious way a continuum of the decade of the 1930s. Clearly the authors are determined to overcome the tradition that has encumbered

civil war studies (for quite explicable reasons), that is, the analysis of the Republican defeat and Nationalist victory from the perspective of the present and in terms of their significance for politics today. One means to this end is the authors' temperate language, in stark contrast with that of civil war protagonists who used a language apocalyptic on the one hand and instrumentalist on the other as both sides manipulated words in order to win allegiance in what was probably the most ideological and political of wars in modern European history. Historians in succeeding decades have, unfortunately, continued to use that language, in part because the issues continue to be relevant.

These authors employ yet another means: the incorporation of social science methodology as a way to analyze the dynamics of a chronology that has by now been fairly well established. Most successful is the essay by Manuel Tuñón and Paloma Tuñón on mechanisms whereby the core ideas in the ideology of each zone were "transmitted" to the mass culture.

One major issue is only obliquely addressed: the need (to use the apt terms of Murray Sperber) to re-create the war's apocalyptic mood for the purpose of analyzing both why and how it "affected" the masses as well as politicians and intellectuals, all the while knowing as we do that "the millennium did not descend." This task will require demythification of a supposedly generic Spanish revolutionary whose most familiar figure is the anarchist. Julio Aróstegui's very good treatment of the social and political components of the war reflects this perspective. Aróstegui describes the direct struggle for control of the workplace between anarchist and socialist unions, a struggle that has always existed distinct from the fight for power between anarchist and socialist politicians.

Aróstegui also best reflects another theme that informs all the essays: the projection of the war as a Spanish phenomenon, albeit one decisively affected by the balance of power in Europe. One example is the still intensely controversial issue of Soviet intervention in the internal politics of the Republic. While acknowledging that fact, the authors concentrate on the realities of Republican Spain that made possible Soviet intervention and on describing the ways in which Spanish politicians sought to counter it. For example, Angel Viñas expands his major reinterpretation, based on extensive work in archives, of the strategies adopted by Spain's politicians in dealing with the realities of contemporary European power politics. Some will debate Viñas's interpretation of Negrín's decision to send Spain's gold reserves to the Soviet Union, but they cannot fault his scholarship. Viñas's perspective should not be dismissed as nationalistic; it offers a healthy corrective to versions that concentrate on the policies of foreign states in a Spain projected as background scenery.

Gabriel Cardona admirably brings order to battles and campaigns, although he works from a fairly conventional concept of strategy based on military considerations alone. Josep M. Bricall, writing in dispa-

sionate terms about emotional issues such as collectivization, is almost the only scholar studying the very different war economies in the two zones; he emphasizes Catalonia not only because it was the most industrialized Republican region but also because of the unexpected state capitalization that contributed to the long-term restructuring of that region's economy.

Mañuel Tuñón, who as editor begins with a summary essay on the indigenous causes of the war, concludes with a deliberately tentative projection of the impact of the civil war on Spain today. He does not attempt to establish the linkage between the Second Republic and the current parliamentary monarchy.

In summary, a new generation of Spanish scholars provides herewith a major synthesis of what is known about the civil war. But they would be the first to assert that much remains to be done, at home and abroad, in archival research and in new scholarly analyses.

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BRIAN JUAN O'NEILL. *Social Inequality in a Portuguese Hamlet: Land, Late Marriage, and Bastardy, 1870–1978*. (Cambridge Studies in Social Anthropology, number 63.) New York: Cambridge University Press. 1987. Pp. xix, 431. \$49.50.

In this anthropological history of northern Portuguese family and village structures, Brian Juan O'Neill has one major aim: "to banish once and for all the myth of the egalitarian hamlet community" (p. 2). Combining the historian's respect for exhaustively researched cases and the anthropologist's respect for exhaustively theorized cases, O'Neill has plunged deeply into archival sources, especially local parish records, to bring historical thickness to his discussion of the communal land system, cooperative labor, marriage, and inheritance patterns, yet, at the same time, he has produced a sophisticated anthropological interpretation of the ways in which forms of social inequality can be brutally apparent and yet tucked within the folds of an egalitarian ideology that exerts, in the words of Pierre Bourdieu, a "gentle, hidden exploitation" (p. 172).

This book is written as a response to the debate among English-speaking scholars in Iberian anthropology about whether concepts of stratification and hierarchy, which are crucially important in southern Iberian agro-towns, are relevant to northern Iberian communities characterized by strong traditions of corporate governance, communal land use, and equal inheritance. Quite deliberately, O'Neill chose to study a small corporate community in the north with a population of less than two hundred people and found, contrary to previous work that stressed the egalitarian social organization and values of such communities, marked social and economic differentiation and a clear awareness of hierarchy among village people. Rejecting, however, the concept of class as "too broad and too generalizing" (p. 15), O'Neill tries to get at the finer

distinctions of difference that prevail on the microscopic level of a small village community, identifying four basic social groups of small holders and rural proletariats, farmers, large farmers, and proprietors. Rather than dismissing altogether the anthropological romance with the reciprocity and egalitarian cooperation of mountain communities, he brings an ironic vision to the romance by arguing that these are "masks of equality" that villagers don at key moments in the agricultural cycle but are quick to remove in the practice of everyday life. Although the system of turns and exchanges that rotate around the village would seem to imply that all are reciprocating equally, a small holder who threshes for a day and a half to help his neighbor who is a large farmer is giving much more of his time and labor than his neighbor needs to give him in order to thresh his much smaller harvest. With irrigation, there is a similar "misrecognition" (p. 173) of equality; a poor household that has a watering turn of a few hours is obligated to spend exactly the same amount of time cleaning the ditches as is a proprietor household possessing larger gardens and a watering turn of several days. Struck by these "collective lies," O'Neill attempted to have villagers confirm his theoretical perspective but admits that, although he "pestered villagers with questions concerning these unequal exchanges," he could not get them to budge beyond saying "that these differences were tolerated" (pp. 171–72).

Having revealed inequalities on the community level, O'Neill moves in the second part of his book toward inequalities at the household level, making connections between small landholders, unprivileged heirs, lower social status, and the marked presence of illegitimate offspring. For the wealthy, patterns of late marriage and elaborate financial negotiations prevail, while, among the poor, marriage is de-emphasized and consensual unions and matrifocal households are commonplace. O'Neill is especially concerned to show how generations of "bastard-bearing women" (p. 253) are reproduced within a social structure that consigns them and their offspring to the lowest place in the hierarchy. Although he acknowledges the sexual exploitation of poor servant women by wealthy men, especially in the past, O'Neill argues that it is, rather, the inheritance system that links together, through illicit unions, excluded male heirs from wealthier families with poorer women from the marginal ranks of the rural proletariat. The equality of heirs is the ideal, yet family inheritances are in practice not divided equally in order to prevent the dissolution of the patrimony. It is the secondary heirs who then become "the 'unknown fathers' of bastard children" (p. 257), reproducing illicitly outside of the household in order to maintain production and respectable, but not excessive, procreation of heirs within the household. Severing the links between marriage, property, and sexuality, secondary heirs are continually moving downward in the hierarchy as they are pushed out to make room for a small elite empowered by patrimony and prestigious matrimony. The

result is a social caste of "bastards," known as *zorros*, an ambiguous derogatory term meaning both "rascal" and "fox."

No one will doubt that this serious, well-argued book makes important contributions to family history, rural history, and European social history, while carrying the debate about Iberian corporate communities to its most intellectually provocative extreme yet. A number of basic questions, however, remain unanswered. Why has the cultural ideal of equality been so pervasive in northern Iberian communities? Is it enough to unveil hidden exploitative aspects of a social system to explain why people, from their different perspectives within a hierarchy, subscribe to such a system? It seems to me that a fuller interpretation would have to take into account the subtle relationship between consensus and consciousness, as well as that between hegemonic systems and everyday forms of discursive and practical resistance. Finally, in terms of feminist history, or at least history that, in the words of Joan Scott, makes gender a useful category of analysis, this book is disappointingly limited. I, at least, am left wondering whether terms like "bastardy" and "bastard-bearing women" can, or should, be used as value neutral in historical and cultural analysis. "Illegitimacy," after all, is a social concept that derives its relevance from a patriarchal church and state that defines as "legitimate" only those offspring properly sanctioned by the law and the cross. To say that "bastardy" or "illegitimacy" serves a certain purpose in a hierarchical social structure and in an inheritance system really does not go far enough toward deconstructing such concepts and, from an anthropological perspective, tells us little about the way the actors themselves invest these concepts with meaning. O'Neill leaves us, in the end, with structures that seem to dovetail too easily, too unresistingly, too phlegmatically, with agency.

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GEORGE ALTER. *Family and the Female Life Course: The Women of Verviers, Belgium, 1849–1880*. (Life Course Studies.) Madison: University of Wisconsin Press. 1988. Pp. xiv, 226. Cloth \$39.50, paper \$24.25.

This book is a quantitative study of females in the Belgian textile city of Verviers. George Alter has joined the family history debate by rejecting the customary unit of study in favor of female family members. By focusing on individuals within families, Alter hopes to offer "a new perspective on the tension between individualism and family values" that will demonstrate the "influence of parents and siblings on the lives and actions of women" (p. 196).

Using sophisticated quantitative methods, Alter has calculated a variety of demographic information. The book contains data on female ages at marriage (women married at a mean age of 25.8), ages at widowhood

(which varied according to occupation and duration of work), numbers of single women (more than one in four females in Verviers remained single throughout life), and fertility (Alter does not define "fertility" so that it is unclear whether his tables show pregnancies or live births). Alter has also investigated the extent to which females who had grown up with both parents in the household tended to marry as compared with females who had lived with only one parent. He concludes that women who had lived with only one parent were likely to remain unmarried. Alter shows that the women of Verviers used birth control but that they also reacted to frequent infant deaths by having another child. That strategy, he suggests, shows that families in Verviers continued throughout the century to depend on children's wages to help support the family.

As with most quantitative studies of families, there are problems here. Most important is the fact that statistical information is used to support broad generalizations about motivation. For example, families are said to have remained strong simply because numbers show that unmarried adult children remained living with parents. No information about the costs or availability of housing in Verviers appears to suggest a different interpretation. Then, too, class is almost entirely absent from Alter's portrait. Numbers about fertility, age at marriage, and so on muddle together all females in the town. Moreover, the numbers are never shown to have changed over time. Thus, readers see only a highly static, almost completely classless society in which economic forces, changing over time, play virtually no role.

Alter has also used his numbers to support stereotypes about women's behavior. Thus, for example, he claims that females in Verviers lived sex-segregated lives. If they did work, they still inhabited a "female" world, both because textile manufacture was "traditionally" women's work and because they worked in sex-specific occupations inside mills. Those working women who did not work in textiles, moreover, worked in other "female" occupations. Unfortunately, that assertion is wrong in the case of Belgium. There women worked in every major industrial (as well as artisanal and agricultural) occupation, including coal mining, glass and armaments manufacturing, and brick making. They were also notoriously good workers—as many popular sayings of the time attest. "Women's work" in the Belgian context, whatever the case elsewhere in Europe, bore very little relation to the 1950s stereotypes of Mrs. Cleaver's job preferences.

Alter concludes, moreover, that women's putatively sex-segregated lives meant that they took no part in public life. Here again, however, his narrow focus on numerical data has led him astray. Far from resembling Jean François Millet's silent, domesticated French peasant woman (depicted on the book's cover), women workers in Verviers were among the most militant in Belgium, as any close reading of archival sources or newspapers shows. Not only did they participate as

both members and militants in the First International, but they also held numerous meetings for women only during the 1870s, where issues of women's emancipation were discussed. Such meetings took place in the ubiquitous workers' café-bars, from which working women were never excluded in Belgium, whatever the case elsewhere.

Given those and other problems, this book should be read with caution. The numbers are, no doubt, useful. But they tell only a limited tale, one that does not support a case about the motives and behaviors of women in Verviers. A more thoroughgoing history of Alter's subjects might use these numbers but would also depend on a variety of impressionistic sources, including hundreds of art works and photographs of the working women of Wallonia. Had Alter looked at those sources, he might have found a picture of Belgian textile workers that was rather different from the passive French peasant woman on the cover of his book.

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L. DE JONG. *Het Koninkrijk der Nederlanden in de tweede wereldoorlog* [The Kingdom of the Netherlands in the Second World War]. Volume 12, *Epiloog* [Epilogue]. In two parts. Leiden: Martinus Nijhoff. 1988. Pp. xii, 648; 649–1208.

When L. de Jong began his work in 1955 on the history of the Netherlands during World War II, he hoped to be able to finish his task within fifteen years and write an account of six or seven volumes. Yet he soon discovered that he needed more time and more volumes to accomplish his task. The volume under review constitutes the twenty-fifth and twenty-sixth parts of his total work. Two more volumes will complete the series, one of which appeared at the end of 1988. In volume 13, the author discusses corrections and sources and provides the reader with a comprehensive index to all volumes. The final volume (number 14) will consist mostly of criticisms offered before and after the publication of each book in the series. Finally, there are plans for English and German translations.

Volume 12 is longer than an average epilogue. In fact, it is a history of the Netherlands during the immediate postwar years and a rather lengthy account of the events in Indonesia from 1945 to 1950. If the period of the German occupation was traumatic for the Netherlands, the immediate postwar years were also trying and complex. De Jong discusses in considerable detail most of the immediate postwar problems, such as the return of prisoners of war and the forced laborers from Germany, the economic and financial chaos, the return of confiscated property to Jewish and other citizens, the role of the resistance in the postwar era, the restoration of political institutions, the trial and punishment of collaborators and supporters of the detested Nazi regime, and so on. After having read the

author's fascinating and detailed treatment of these and other, often enormously complex problems, I could not help but sympathize with Dutch authorities who had to find so many equitable solutions. Naturally, they made many errors, but de Jong tempers his judgment with understanding and empathy.

One of the most interesting and important parts of de Jong's work is his lengthy discussion of the complex issue of prosecution and punishment of war criminals, Nazi sympathizers, and collaborators. Unlike France, where we witnessed much arbitrary "justice" in the days right after the liberation, the Netherlands did not experience much popular violence against disloyal citizens. Yet de Jong graphically describes how many were imprisoned under inhumane circumstances and brutally treated by their guards. Many were released in the summer of 1945, while some fifty thousand were tried by tribunals and more serious cases by special courts of justice. In many cases the death penalty was demanded and carried out, although the Netherlands had abolished capital punishment long before World War II. Among those sentenced were a number of German war criminals, two of whom were imprisoned until 1988. The attempt to purge and punish members of the civil service, the police, the business world, the press, the artistic community, the universities, and other professions proved to be much more difficult than had been assumed because it was soon realized that to some degree all Netherlands had collaborated with the German authorities.

The author often refers to the disillusionment of many Dutch citizens over the failure to effect "renewal" in the postwar years. Hopes were high that the old political parties would not return and that Dutch society would be less divided into *zuilen*, or columns, such as Catholics, Protestants, and socialists. Perhaps this wartime idealism was unrealistic. Yet there were important social and cultural changes in the 1960s, although the author is reluctant to attribute those changes to wartime experiences. Especially disillusioned were many members of the organized wartime resistance who had hoped for a thorough renewal of Dutch society. Although there was and still is much public recognition and appreciation of the work of the resistance, most Dutch citizens felt that the norms and values of this group of heroic individuals were too rigid and not applicable. To Queen Wilhelmina, however, the resistance remained the embodiment of real patriotism and heroism. Yet, in spite of the disillusionment and failures of the postwar era, the Netherlands did succeed in quickly rebuilding its shattered economy, albeit with American aid from the Marshall Plan, and in restoring democratic institutions.

Most of part 2 of this volume discusses the decolonization of Indonesia and provides us with one of the most comprehensive recent accounts of this postwar drama. Here, as elsewhere in volume 12, the author often relies heavily on other published works and documents. It is unfortunate, however, that he did not consult many of the yet unpublished documents of the

period 1948–49. Perhaps no other issue stirred and divided the Netherlands in the postwar years more than its conflict with the Indonesian republic. Although the Dutch were rather successful in recuperating from the ordeal of German occupation, they stumbled in effecting a swift and painless decolonization of their empire in the Far East. De Jong does not give us any new explanations of why the Netherlands failed, but he correctly assigns much of the blame to the Roman Catholic political leadership. Any student of World War II must read de Jong's impressive work. Let us hope that we may soon have a condensed English translation of his magnum opus.

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HANNU SALOKORPI. *Pietarin Tie: Suomalainen puolue ja suomettarelainen politiikka helmikuun manifestista Tarton rauhaan 1899–1920* [The Road to St. Petersburg: The Finnish Party and the Policy of Its Leaders from the February Manifesto to the Peace Treaty of Tartu, 1899–1920]. Summary in English. (Historiallisia Tutkimuksia, number 145.) Helsinki: Suomen Historiallinen Seura. 1988. Pp. 358.

Hannu Salokorpi's dissertation is a study of the leadership of the Old Finnish party and its policies in the first two decades of the twentieth century. The author argues, although not entirely convincingly, that, despite seeming contradictions and some shifts, Old Finn policy in the years from 1899 to 1920 formed a "consistent whole" (p. 20). The "road to St. Petersburg" symbolizes this alleged continuity. In practical terms the policy involved seeking compromise and cooperation with tsarist Russian and Soviet authorities. In the russification era before 1905, the party, hoping to ward off more sweeping attacks from St. Petersburg, achieved notoriety with its policy of compliance. Nevertheless, the Old Finns became the largest nonsocialist party (fifty-nine of two hundred seats) in Finland's first democratic parliament in 1907. The party's new platform, appealing both to Finnish nationalism against the Swedish-speaking elites and to the lower orders through social radicalism, no doubt contributed to this success. In the decade before the Russian revolution, the Old Finns proved less accommodating to pressure from St. Petersburg, but they still hoped for a compromise solution to the problem of Finnish-Russian relations.

Salokorpi's continuity thesis remains credible through 1917 as the Old Finns only reluctantly supported Finnish independence and sought to maintain some ties to Russia. Bolshevik success in Russia, however, and the bitter Finnish civil war overturned traditional approaches. In an abrupt about-face, the Old Finns by April 1918 supported an alliance with Germany (and a German prince for a new Finnish monarchy), limitations on democracy at home, and defusion of the domestic language struggle between Finnish and

Swedish. Here the "road to St. Petersburg" breaks down. More accurately, the real continuity in Old Finn thinking was the primacy of foreign policy. The party leaders always viewed domestic politics through the prism of the security issue. The Old Finns' shift to Germany was a desperate search for protection against a seemingly hostile Soviet Russia. Germany's role as an ally, however, collapsed with the end of World War I, and the party returned to its traditional Russian emphasis, taking the lead in peace negotiations with the Soviets.

Salokorpi concentrates on the "inner circle" of Old Finns, especially J. R. Danielson-Kalmari, J. K. Paasikivi, Lauri Ingman, and Ernst Nevanlinna, and provides useful detail from archival sources on their views. The book, however, is long on description and short on analysis and offers a narrow view of the party. We learn almost nothing of the party's structure or social base and very little of the elite's interaction with the rank and file. The Old Finns suffered almost continual electoral decline between 1907 and 1919, but the impact that decline may have had on party policy is hardly broached. The "road to St. Petersburg" is approached only through Finnish sources and merely serves as a metaphor; contacts between Old Finn leaders and tsarist or Soviet authorities are not explored. The book includes a ten-page English summary, but its value is limited by a shaky command of the language.

Perhaps the most valuable part of the book is Salokorpi's delineation of the Old Finn role in the origins and emergence in December 1918 of the National Coalition party. The new party was a conglomeration of frustrated monarchists from the Old Finns, the Young Finns, and the small People's party, who had little in common ideologically. In the interwar era, the traditional Old Finn position faded into the background in the face of dominant anti-Russian attitudes, but it made a triumphant comeback after World War II and formed the basis for postwar Finnish foreign policy under the Paasikivi-Kekkonen line.

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STEFAN FISCH. *Stadtplanung im 19. Jahrhundert: Das Beispiel München bis zur Ära Theodor Fischer*. Munich: R. Oldenbourg. 1988. Pp. xxii, 329. DM 78.

In the history of cities, one of the crucial questions is who plans? The liberation of the peasantry and the influence of the French revolution had established the principle that landowners were free to develop their property as they saw fit, yet during the nineteenth century industrialization and the associated rapid growth of urban populations forced politicians and architects to find means to control and channel development in order to avoid chaos. Stefan Fisch presents influences on the planning process in Munich and recounts key steps in its development. That planning

process is still in effect today, and it continues to create and maintain one of the most attractive urban environments in Europe.

Fisch's book illuminates in significant detail a century-long series of problems, sociopolitical conflicts, and planning solutions for one specific case. The Munich example, however, is of far more than local interest, since it incorporates considerations of significance to all major cities and since the solutions found produced an exemplary urban community. But communal planning in Munich was realized only at the end of the century during Theodor Fischer's reign as head of the urban expansion commission from 1893 to 1901.

Fischer, an important pioneer in the development of city planning, greatly admired the work of the Austrian author Camillo Sitte. Fischer opposed mechanistic application of gridiron plans to urban development and proposed bending the axes of some streets (for example, Arnulfstrasse) to achieve meaningful focal points and a sequence of clearly perceptible street spaces. Even more important, the ideas of Sitte and Fischer demonstrated possibilities of conceiving and planning an urban environment in its entirety so that all citizens, not just the land developers, could benefit.

The struggle for urban planning, well documented in this book, was fiercely contested throughout the nineteenth century. Munich, like so many of its sister cities that were characterized by developing industries and rapidly growing populations, presented an offensive, unhealthy image to residents and visitors alike. Recurring typhus and cholera epidemics and the ongoing irritants of dust, mud, and horse manure demonstrated to everyone the need for a sanitary water supply, effective waste water removal, and paved streets; but property owners resisted having their rights curtailed or their taxes raised to pay for public improvements. Financing such projects and obtaining the consent of landowners through compensation or other means made the contributions of urban jurists as important as those of administrators and architects.

The fruition of a century's development in Munich was the zoning ordinance of 1904. Because many citizens and developers viewed the idea of governmental direction of urban development as anathema, the passage of an ordinance reserving public authority to regulate development on privately held land was epoch making. Planning regarding heights of buildings, degree of openness, intensity of use, distribution of green open spaces, and harmonious integration of functions became possible through the application of political astuteness, new legal procedures, and artistic vision.

Fisch has convincingly documented the historic progress from laissez-faire liberalism to a highly successful, planned urban agglomeration. He was awarded the 1986 Urban Science Prize of the Foundation for German Cities and Communities for this book.

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HARRY LIEBERSOHN. *Fate and Utopia in German Sociology, 1870–1923*. (Studies in Contemporary German Social Thought.) Cambridge: Massachusetts Institute of Technology Press. 1988. Pp. x, 282. \$27.50.

This book is well written, concise, and illuminating. It contains an analysis of five German sociologists: Ferdinand Toennies, Ernst Troeltsch, Max Weber, Georg Simmel, and Georg Lukács. It is not, however, a review of their main substantive work, though Harry Liebersohn draws on that work for purposes of his own. The title suggests that these originators of German sociology counterposed their pessimistic views of modern society with ideas of the human potential, here called fate and utopia. They were not simply empirical social scientists, although that was part of their intention as well. To see both of these sides is apparently difficult, and Liebersohn does not attempt it. But, in view of many American interpretations of these writers, it is important to show that the "founders" were engaged in a major effort to preserve or advance the spiritual legacies of European humanism.

A generation ago, Bennett Berger and I pointed out that images of society are at the basis of all work in the social sciences. In the absence of attending to this perspective, the social sciences, and particularly sociology, tend to split into humanistic and empirical "factions" with little contact between them, I think to the detriment of both. In an illuminating conclusion, the author states that one must listen closely to find the half-hidden religious image that these German writers opposed to the burdens of the present. "Their hopes tended to surface in association with a preferred religious tradition: for Toennies, the 'Third Age' of Joachim; for Troeltsch and Simmel, German mysticism; for Weber, the voluntary associational life of ascetic Protestantism; for Lukács, Russian mysticism" (pp. 195–96). It takes discernment to discover the common core in these divergent affinities.

Liebersohn does so by analyzing Toennies's clear preference for community over society, the different attempts of Troeltsch and Simmel to balance their appreciation of past stabilities with a more positive approach to modernity, Weber's attempt to counterpose the value of personality against the threatening force of bureaucratization, and, finally, Lukács's rejection of society in favor of a projected communitarianism. This list is obviously a mere summary of a complex argument. One can quarrel about detail: I for one would have found it illuminating if Liebersohn had attempted to search for Toennies's images of community in his important criminological studies, for Simmel's preoccupations in his analysis of Kant (or again of Rembrandt), or for Weber's views of personality in his work on ancient Judaism. But these are small drawbacks compared with what the volume accomplishes: the intellectual restoration of Lukács to his significant position among these four German scholars.

The reference is to the early Lukács, especially his history of the drama and his theory of the novel.

Although it was well known that Lukács studied with Simmel and was influenced by Weber, the author's perspective enables him to show that Lukács made an original contribution to the analysis of modernity, properly placing him among the other four, even though his early work was a prelude to his later "idealistic Marxism." The author does not say so, but his assessment of the early Lukács sheds much light on the work of Max Horkheimer and Theodor Adorno, the founding members of the Frankfurt school, who drew on that "idealistic Marxism" for their critical theory but did not follow Lukács's subsequent identification with Russian culture (Fyodor Dostoyevsky) and the Soviet Union. In any case, Liebersohn's interpretation of the early Lukács is a valuable contribution to our knowledge, facilitated by the recent translation of several of Lukács's early works from the original Hungarian into German and English.

One unresolved theoretical issue remains. If it is true, as this study shows convincingly, that these founders of German sociology had a moral-philosophical agenda in pursuing their studies, then what is the relation between these studies and that agenda? And can one analyze that relation without an agenda of one's own? The value of Liebersohn's study is measured in part by the fact that it leads us to these central questions.

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DETLEV J. K. PEUKERT. *Jugend zwischen Krieg und Krise: Lebenswelten von Arbeiterjungen in der Weimarer Republik*. Cologne: Bund: 1987. Pp. 372. DM 48.

This book is the companion volume to Detlev J. K. Peukert's *Grenzen der Sozialdisziplinierung* (1984). In that volume Peukert examined the approach taken by social workers, educational theorists, and criminologists between 1878 and 1932 to the "problem" of youth; in his second book, Peukert attempts to give an account of the lives of working-class boys and young men during the Weimar Republic. As Peukert points out, a study that included girls and young women would have required a more wide-ranging and complex research strategy than the one he has employed. In this volume, he relies on essentially the same sources that he used in the first book, namely, the published accounts of youth professionals, observers who were not particularly worried about girls. In the treatment of the sources, too, Peukert's work in this book resembles his work in the previous volume; the account consists largely of a presentation of the findings of contemporary observers, involving extensive direct quotation from published material against a background of very enlightening and accomplished analyses of demographic and employment statistics.

A research strategy of this kind is bound to make other historians uncomfortable. On the one hand,

studies of youths written during the Weimar years were often based on questionnaires or surveys in which young people were invited to write about themselves; those studies included extensive quotations from the essays elicited and unquestionably constitute an important source of primary material. On the other hand, contemporary writers were necessarily selective in their presentation of texts. Some of the most tantalizing sections of their work consist of statistical analyses of the content of statements that they do not quote at length, and Peukert's citation over several pages of the results of their reading and their taxonomies will leave many readers yearning for evidence of an independent set of sources against which these secondhand observations may be tested.

That said, and leaving aside the question of whether it would be possible to find such sources, this book is a considerable achievement of research and imagination. By bringing together the results of those contemporary studies, which other historians have cited but never so comprehensively exploited, it fills an information gap, but Peukert is also critical and intelligent in his deployment of his sources and always sensitive to the possibility of alternative readings. His concern, moreover, is to produce a synthesis, a general picture of the situation of working-class youth, and in this he is successful, in spite of a simultaneous insistence on the variety of youthful experience to which he is compelled by his sources. Examining in turn the demographic context, socialization within working-class families, work, leisure, deviance, and young people's self-image, he makes the following general points: young people's vision of themselves and their world in the 1920s and early 1930s was characterized by an increasingly general uncertainty about the future. That view reflected the economic instability of the period, from which young people suffered disproportionately (not least because there were more of them than at any other period in modern German history). But uncertainty was also related to the programmatic openness of Weimar society. At the same time, young people showed a remarkable capacity to cope with these stresses, as revealed in the "processing" of experience through integrative subcultures and the low rate of juvenile crime during the depression. In turn, their own attitudes and subcultural practices emerge as the products of a particular historical conjuncture, in which elements of a traditional family- and work-centered identity combined with those of a modern youth culture, for example, the extension to young workers of the concept of a transitional phase between (dependent) childhood and (wage-earning) adulthood, the popularization of the idea of "leisure," and the growth of a commercialized leisure industry.

EVE ROSENHAFT
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KARL MARTEN BARFUSS. *"Gastarbeiter" in Nordwestdeutschland 1884-1918*. (Veröffentlichungen aus dem

Staatsarchiv der Freien Hansestadt Bremen, number 52.) Bremen: The Archive. 1986. Pp. 294.

ADELHEID VON SALDERN. *Auf dem Wege zum Arbeiter-Reformismus: Parteilalltag in sozialdemokratischer Provinz Göttingen (1870–1920)*. (Geschichte sozialer Bewegungen, Materialis Programm, number 27.) Frankfurt am Main: Materialis. 1984. Pp. 345.

These two excellent monographs show us why labor historians should look at the geographic, social, and political peripheries of the Wilhelmine labor movement as well as at its major strongholds. Karl Marten Barfuss presents a fascinating picture of the Bohemians, Poles, Ruthenians, and Croats who came from Eastern and Southern Europe to find jobs in the jute, cotton, shipbuilding, construction, brick-making and shipping industries of northwestern Germany. Adelheid von Saldern tackles a more traditional subject, the history of the Social Democratic party (SPD). Yet the very unusual sources she has unearthed—the minute books of the Göttingen party branch meetings—along with the provocative questions she asks make this book an exceptional study of the party “from below.”

Caught in a vice between the high wage demands of local German workers and the pressures of fierce foreign competition, employers in northwestern Germany sought to keep wage bills low by recruiting a succession of poorly paid “foreign” workers, especially young women, from backward regions. The locals viewed these new arrivals as cultural inferiors who accepted wages and working and living conditions that no self-respecting German worker would tolerate—“little Cameroon” (*Klein Kamerun*) was one of the designations for neighborhoods where foreign workers predominated. German socialists and trade unionists complained about the absence of class consciousness among foreign workers, while employers praised their loyalty. But Barfuss dispels many of the stereotypes that supported the Wilhelmine discourse on “foreign labor” by examining in detail the economic, legal, and political constraints shaping the immigrants’ responses. Initially, immigrant workers were willing to accept low wages and bad working conditions without protest because there was a significant difference between even the lowest wages paid immigrants in northwestern German industry and what they could earn at home. The lives of the foreign workers were tightly controlled by employers. Company housing could resemble a prison camp with dogs and armed guards. Resistance produced quick deportation. Such a disorienting and hostile environment initially encouraged only passivity or unorganized crime and violence. But the behavior of foreign workers did change; they began to take part in strikes and to join trade unions. Like their counterparts in the Ruhr, Polish workers also developed a strong national consciousness and a thriving national subculture centered on the Catholic church and a variety of clubs and associations. And yet this did not prevent the Poles from realizing that their political interests were best served by voting for the Social Democrats rather

than for the Catholic *Zentrum* or for a Polish nationalist party. But the Bremen Social Democrats unfortunately managed to alienate foreign workers after the turn of the century when the local party branch became more radical and embraced Rosa Luxemburg’s anti-Polish nationalist stance. And both trade unionists and Social Democrats found themselves locked in a blatant contradiction between their rhetorical commitment to internationalism and their actual fears of cheap foreign labor.

The ethnic working-class cultures of northwestern Germany were by-products of a developing world market and the international movement of labor. The lives of the Göttingen Social Democrats, on the other hand, were circumscribed by their structural isolation from such large-scale economic processes. Stuck away in this “proletarian province,” weak in numbers and resources, deprived of direct experience of the major capitalist developments and of the dramatic political conflicts of their time, the members of the Göttingen SPD felt that they had to live a vicarious political life. Von Saldern’s close analysis of the Göttingen party minute books shows that there was much discussion of national and international events at party meetings but little interest in local matters. The Göttingen Social Democrats made no serious attempt to integrate the existing informal structures and practices of the regional plebeian and working-class cultures into the everyday life of the Göttingen party branch, and they failed to build a vital regional popular movement or to construct an alternative, oppositional “proletarian public.” As a result, they were ill-prepared to exploit the new openings in local politics that came with the German revolution of 1918–19.

Some of the questions that von Saldern has formulated would probably not have been asked by a West German historian before the entrance of the Greens onto the German political stage or before the current upsurge of interest in the “history of everyday life” (*Alltagsgeschichte*). And Barfuss’s analysis of the Wilhelmine immigrant experience has undoubtedly benefited from recent discussions of the *Gastarbeiter* problem in the Federal Republic. In its own way, each book shows that history can draw fresh insights from contemporary political preoccupations without becoming uncritically enslaved to them.

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DONALD J. DIETRICH. *Catholic Citizens in the Third Reich: Psycho-Social Principles and Moral Reasoning*. New Brunswick, N.J.: Transaction Books. 1988. Pp. xii, 356. \$39.95.

Of all the imponderables associated with Nazi Germany, few are more haunting than the near silence of the church in the face of the Holocaust. Over the past generation, scholars have made any number of at-

tempts to explain why the institution most successful in avoiding Nazi *Gleichschaltung* should also have been so unable or unwilling to oppose the Third Reich's genocidal policies. Donald J. Dietrich's study of German Catholics seeks to advance this discussion by analyzing Catholic attitudes and behavior from a social-psychological perspective, particularly that suggested by recent cognitive and social exchange theory. The result is a thoughtful and sometimes stimulating account that both recalls and goes beyond the classic work of Guenter Lewy, Gordon Zahn, and others.

To be a German Catholic, Dietrich argues, was to live within the framework of two potentially incompatible value clusters, one Christian and humanitarian, the other nationalist and racist. How the church dealt with resulting forms of cognitive dissonance depended largely on its specific historical environment, and, in Dietrich's view, virtually every aspect of pre-Nazi Catholic experience, both individual and collective, conspired to resolve moral tensions in favor of mainstream German nationalism. The first half of the book makes a detailed case for the proposition that national unification and the Kulturkampf of the 1870s encouraged most German Catholics to embrace the authoritarian and antiliberal values of the Kaiserreich as a way of escaping isolation in an alien Protestant culture. In addition to blurring distinctions between religious and patriotic norms, this compulsion to compensate for their own minority status also had the effect of desensitizing Catholics to fellow minorities, especially Jews; indeed, anti-Semitism was a ubiquitous and politically malleable tool for Catholic social integration.

To the extent that the Hitler movement also enveloped itself in traditional nationalism, Catholics therefore lacked any overriding cognitive necessity to develop an unambiguous theological or political critique of the party's *Weltanschauung*. While Dietrich credits German bishops with considerable realism about Nazi intentions, both before and after 1933, he supports the contention of many previous scholars that the church's typical modes of response—tending as they did to emphasize legal niceties and couch protest in conventional patriotic rhetoric—were ill-suited either to provide moral guidance or to spark active resistance among the laity who, for all their loyalty to the clergy and to Catholic teachings, could hardly be expected to oppose the regime “unless local parochial interests could somehow have been given universal meaning” (p. 297). This complex failure to distinguish national from universal meaning found its ultimate and most disturbing consequence in the Holocaust, the implementation of which depended as much on mass indifference as on active racial hatred.

Dietrich offers a careful reconstruction of the psychological dynamics that may have undergirded the apparent cultural captivity of most Catholics in the Third Reich. His interpretation is noteworthy for rejecting once-fashionable psychoanalytic models as inadequate and inappropriate; his flexibility in appropriating clinical constructs stands in marked contrast to

some of the more overweening products of old-school psychohistory. And concern with theory does not lead Dietrich to devalue traditional empirical research, as his copious notes and forty-page bibliography amply attest. To be sure, the marriage of techniques is not without its strains. Despite moments of genuine eloquence, the book is hardly a model of felicitous style or organization; too often the prose calls to mind Frank Manuel's well-known caveat about social science jargon, that is, that its sheer ugliness ought to render it unfit for service in historical narrative. Dietrich never fully resolves an implicit tension between his acceptance of a basically intentionalist-totalitarian model of the Third Reich and his repeated insistence that the church's influence on public opinion represented a political force that, if consciously exploited, “could have played a positive role in averting the Jewish disaster” (p. 232). Given Dietrich's reliance on social exchange theory, one might also have expected him to discriminate more systematically among the variety of different classes, regions, and types of social milieu within the Catholic subculture.

If, in the end, Dietrich's argument seems more plausible than compelling, this aspect does not seriously detract from his achievement. His book not only offers a balanced synthesis of earlier scholarship but also suggests new ways of thinking about familiar issues. As such, it constitutes a worthy addition to the ever-growing list of major works on the church struggle.

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ROBERT PROCTOR. *Racial Hygiene: Medicine under the Nazis*. Cambridge: Harvard University Press. 1988. Pp. viii, 414.

The involvement of physicians in the crimes of the Nazi regime has been known since the Nuremberg medical trial, but until recently scholars have paid scant attention to the role of medicine in Nazi Germany. The last decade, however, has brought a change. Historians in North America, such as Michael Kater and Geoffrey Cocks, have dealt with the medical profession and its practitioners during the period from 1933 to 1945, and research in Germany by Götz Aly, Gerhard Baader, Karl Heinz Roth, Heidrun Kaupen-Haas, and others has focused on the involvement of the medical community in Nazi crimes. Most important, Benno Müller-Hill has shown in his *Tödliche Wissenschaft* (1984) how biomedical science legitimized Nazi crimes and how geneticists, anthropologists, and psychiatrists initiated, executed, and profited from those crimes. Robert Proctor's study is a welcome addition to that literature. It is based on his careful and time-consuming reading of the unusually large number of German medical journals and scientific books; secondary works and selected archival sources supplement his research.

The central theme of Proctor's study is an analysis of

a success story: the triumph of racial science (*Rassenhygiene*). He traces racial science, the German version of eugenics, from its origins during the late nineteenth century to its prominence during the Nazi years, showing how eventually its ideology permeated all aspects of German medicine. At the beginning the German eugenicists, including Alfred Ploetz and Wilhelm Schallmayer, did not subscribe to the anti-Semitic cult of the Nordic race, although their attitude toward blacks would certainly stamp them as racists today. But during and after World War I, the German eugenic movement absorbed the Nordic racial ideology.

Proctor does not focus on how the Nazis corrupted science but rather on "how the [biomedical] scientists themselves participated in the construction of Nazi racial policy" (p. 3). Proctor shows how Nazi physicians such as Arthur Gütt, Walter Gross, Gerhard Wagner, and Leonardo Conti, who designed and implemented the racial legislation of the Third Reich, only applied the scientific principles established by such leading racial scientists as Fritz Lenz, Eugen Fischer, Ernst Rüdin, and Otmar Freiherr von Verschuer. He demonstrates how the adoption of racial science by the Nazis, and the adoption in turn by scientists and physicians of Nazi ideology, led first to large-scale sterilization and eventually to the murders of all those considered genetically diseased. Most important, Proctor shows how the values of eugenics adopted by the Nazi movement were shared by the international scientific community (even in the United States).

Proctor's study is not a political history and does not attempt to delineate the political alliances or administrative structures of the German health care system. Small errors—for example, Philipp Bouhler headed Hitler's personal chancellery and not the party chancellery (pp. 182, 192); the Wannsee protocol was discovered in the files of the Foreign Office and not in those of the SS (p. 210)—thus do not invalidate his argument. Instead, Proctor's book is an intellectual history that probes the scientific origins of Nazi racial policies; political events and administrative procedures are presented only as illustrations to show the victory of an idea.

Proctor makes two important points. First, he rejects the conventional wisdom that the anti-intellectual Nazi movement corrupted and devalued science. Instead, he points out that Nazism "appealed to academics and intellectuals" and was able "to draw upon the imagery, results, and authority of science"; further, he demonstrates that "Nazi ideology informed the practice of science" (p. 283). Second, Proctor demonstrates the connection in Nazi ideology, as well as in racial science, between the exclusion of the handicapped and the expulsion of the Jews. He shows how "euthanasia" and the "final solution" were viewed as part of "a common program of racial purification" (p. 221).

This is an important book. It provides for us new evidence on the roots of Nazi criminality, showing how an all-encompassing program based on racial science

led in Nazi Germany to the murder of the unwanted and the alien.

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LOTHAR GRUCHMANN. *Justiz im Dritten Reich 1933–1940: Anpassung und Unterwerfung in der Ära Gürtner*. (Quellen und Darstellungen zur Zeitgeschichte, number 28.) Munich: R. Oldenbourg with the cooperation of the Institut für Zeitgeschichte. 1988. Pp. xxxviii, 1297. DM 198.

Notwithstanding its all-encompassing title, this massive tome focuses on one central aspect of the Nazi judicial system, namely, the accommodation and subjugation of the Reich Ministry of Justice to the regime's totalitarian objectives and demands during the stewardship of Franz Gürtner. In almost one thousand one hundred fifty closely printed pages of text, whose publication was supported by the ministry's successor in the Federal Republic, Lothar Gruchmann traces Gürtner's career from his political debut as Bavarian justice minister at the time of Hitler's abortive 1923 *Putsch* and subsequent trial, through his selection for the same post in the cabinets of Chancellors Franz von Papen and Kurt von Schleicher (1932–33), to his continuation in that portfolio under the Nazis. Gruchmann attributes Gürtner's appointment in the Nazi regime not to the all-too-tolerant treatment accorded Hitler a decade earlier by the authorities in Bavaria but to Hitler's recognition that the conservative nationalist Gürtner was prepared to adapt legal principles and structures to political needs. Once embarked on this slippery slope, Gürtner and the like-minded officials in his ministry soon found to their dismay that the authoritarian, but still constitutional, state they envisaged was to be supplanted by a ruthless dictatorship in which the will of the Führer alone determined what constituted the law, as enforced by the National Socialist party, its affiliated organizations (SA, SS), and newly created governmental institutions (especially the Gestapo). Much of Gruchmann's analysis of the justice ministry's policies in the areas of codification and reform of the penal code and court procedures recounts the ministry's conflicts with such figures as Hans Frank, president of the self-styled Academy for German Law, and political police chief Heinrich Himmler over the degree of nazification that was acceptable or desirable in Germany's legal edifice. Broadly speaking, Gürtner was prepared to compromise almost every civilized judicial tradition, including the inadmissibility of punishment under retroactive laws and even the untrammelled independence of individual judges, in an attempt to preserve at least the outward form of the *Rechtsstaat*. The result, not finally achieved until the war years by Otto Thierack who followed Gürtner in office, was the effective transformation of the legal system into an instrument for perpetuating the regime by sup-

pressing its opponents with the utmost brutality under the thin guise of law.

For the nonspecialist reader, probably the most interesting sections of the book are those dealing with concrete instances of clashes between the inherited practices of German law officers, on the one hand, and Nazi illegality, on the other. Thus, Gruchmann graphically describes the stubborn efforts of a few state attorneys to investigate the bestial mistreatment of prisoners by their guards in various "provisional" concentration camps during 1933 and 1934, interventions that rarely ended in convictions. Even more courageous was the opposition by district court judge Lothar Kreyssig to the implementation of the so-called euthanasia program (that is, the mass killing of the physically and mentally handicapped); his threat to file murder charges against the party leaders responsible eventually helped to throttle that particular crime. But such behavior within the German legal profession nominally headed by Gürtner until his death in January 1941 was not the norm. His assent, albeit sometimes grudgingly given, to the "cleansing" of the profession from politically and racially "undesirable" elements; to the "Lex van der Lubbe," which ex post facto permitted the execution of the Dutchman who set fire to the Reichstag building; to the legalization of Hitler's orders to shoot without trial Ernst Röhm and other victims of the purge of June 30, 1934; to the quashing of indictments against Nazis who took the lives of ninety-one Jews in the course of the Reichskristallnacht pogrom; and to many lesser-known actions that Gruchmann documents maintained the jurisdiction of the ministry as the recognized dispenser of justice in the Third Reich, while simultaneously emptying the concept of its essential meaning.

This study, which is extensively based on archival research, will doubtless stand as a "definitive" reference work for a long while to come. Its evaluations of the persons involved are perceptive and fair, though in the case of Gürtner perhaps overly generous. Written expressly for historians, the book in places is excessively detailed. Its extended table of contents and comprehensive indexes, however, will greatly facilitate usage.

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HENRY M. ADAMS and ROBIN K. ADAMS. *Rebel Patriot: A Biography of Franz von Papen*. Santa Barbara, Calif.: McNally and Loftin. 1987. Pp. 513. \$25.00.

This exhaustive biography of Franz von Papen (1879–1969) is a singularly charitable portrait of its subject. In a book of some five hundred oversized pages, there is virtually no instance when the life of the well-known German officer, politician, and diplomat is examined critically. It is clearly the premise of Henry M. Adams and Robin K. Adams (who identify themselves in the

text as "the writer and his wife") that Papen could do little wrong (pp. 465, 497, 498).

Guided by their premise, the authors portray Papen as a selfless patriot and victim of circumstances who was obstructed by people of ill will. In the process, however, they employ several exceedingly questionable methods to defend him. For example, during Papen's service as military attaché in the United States in the first two years of World War I, he hired agents in the fashion of other military attachés. To the authors this "was all quite natural" (p. 16). But, when Papen's agents were apprehended and their activities were exposed and when Papen himself was implicated (and eventually recalled), the authors rush to his defense. They assert that he was "taken in" by certain agents (p. 19), that Berlin sanctioned his actions as "humane" (p. 50), that he was hounded by "snooping reporters" (p. 58), and that U.S. Secretary of State Robert Lansing, in addition to using "weasle-words [*sic*] of expediency" in the case (p. 65), was subject to "Germanophobia" (p. 72). With the onus thus shifted from Papen, the authors conclude that he was made a "scapegoat for German policy and German actions in America" during World War I and that "the entire Left" contributed to that circumstance (p. 118). It is not a convincing defense, especially since it rests so heavily on recriminations against other historical actors.

Two further examples illustrate the authors' defensive method. First, they try to settle complex issues by using simple entreaties. They challenge the assumption of most historians that when Papen met with Adolf Hitler on January 4, 1933, he intended to undermine the government of his former mentor General Kurt von Schleicher. To be sure, the evidence for the assumption is not entirely conclusive. But the authors refuse even to consider anything but generous motives on Papen's part. They conclude that "one must also believe the ever good-intentioned Papen that he in no way wanted to undermine the cabinet of Schleicher in this contact with Hitler" (p. 201). The unsupported entreaty is simply not persuasive, given the impending demise of the Schleicher government and the emergence of Papen as vice chancellor in the new Hitler government some three weeks after his meeting with Hitler.

Most seriously, the authors tend to skirt central questions while basing their defense on decidedly minor issues. An example is provided by the terrible events surrounding the Night of the Long Knives on June 30, 1934. While Papen was subjected to a brief house arrest orchestrated partly by Hermann Göring, the Nazi fury raged. One of Papen's speech writers as well as his press aide were murdered, and numerous other people whom he knew (including Schleicher) were summarily shot. But what did Papen do? He sought at first to defend his own honor and the innocence of his press aide in personal contacts with Hitler. Shortly thereafter he congratulated Hitler on a "great speech" of explanation for the purge, looked after the financial arrangements of his murdered asso-

ciates, and went hunting (pp. 254–55). Two weeks later he accepted Hitler's offer to lead a special German mission to Vienna. There he represented Nazi Germany until 1938. It is extraordinary that the authors do not at this juncture address the serious and fundamental issue of the extent of Papen's complicity with the Nazi regime. Instead, they dwell on a marginal issue to defend him, focusing on the question of his capacity for hatred. They recount a meeting between Papen and U.S. Ambassador William E. Dodd during which Papen evidently voiced his "hatred" for Göring. This was unlikely, they insist, adding that he "could not really hate, he could only dislike, and for that weakness he paid dearly and it was one of the reasons why he could not extricate himself from continuous misfortune, as though born under an unlucky star" (p. 254). In this fashion, the central question of Papen's complicity with a criminal state is passed over, to be disputed later but no longer in context.

The authors might have based a more plausible defense on the probable fact that, after the Night of the Long Knives, in Vienna, and during his wartime service as German ambassador to Turkey, Papen constantly feared for his life. In a review essay of Papen's *Memoirs* in 1953, Theodor Eschenburg suggested just such a line of defense, albeit half-heartedly. But the authors reject this or any other nuanced interpretation of their subject. Papen, they maintain in the end, was a gentleman. On the way to this barren conclusion they indulge in frequently questionable condemnations of other historians, going so far as to charge that Papen's two books were "systematically hushed up" (p. 506). This assertion is simply not true, as Eschenburg's long review of Papen's first book illustrates. What is true is that the uncritical portrait of Papen, first adumbrated by Papen himself and now enlarged by the authors, does not persuade.

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ARTHUR A. DURAND. *Stalag Luft III: The Secret Story*. Baton Rouge: Louisiana State University Press. 1988. Pp. xiii, 412. \$29.95.

Accounts of war normally provide colorful descriptions of glorious deeds, valorous sacrifice, and ultimate success. Heroes win, losers rationalize. A soldier's death in battle provides medals for family members, posters for fund drives, and material for the media. The sacrifice of life inspires some to emulation and others to exhortation.

For those individuals, caught by circumstances, fear, or the vagaries of battle, who raised their hands in surrender to the enemy comes a black hole in life. They did not die, did not sacrifice, did not pay the ultimate price. Somehow they survived and avoided a reality nonparticipants raise at embarrassing moments. Historians have likewise neglected the issue of being a

prisoner of war—a sad commentary on the reality of wars.

Arthur A. Durand uses a case study of a specific camp in explaining the circumstances of prisoners of war. He has labored diligently and long in pursuing source materials, oral histories, and minute details concerning Stalag Luft III during World War II. The camp, created in 1942 and used until February 1945, was a sprawling complex of compounds and constituencies with over ten thousand prisoners. Durand knows everything about the installation and provides a splendid account of the circumstances confronting the pilots who survived combat in confinement.

Durand's volume demonstrates the difficulty of telling a story after many years of study. His account is exemplary in structure, in sources, in detail. One can only admire his dedication and his research. At the same time, his years of effort create a narrative imbalance. He believes in the unique value and importance of his subject. An example of that commitment is his opening account of individual captures, which he does not weave into the subsequent collective experience. Likewise, Durand enjoys recounting individual experiences and activities that provide excitement but not always explanation. The administrative structure of the camp is unclear; the role of General Arthur Vanaman remains opaque at best.

Historians must welcome a book that addresses the concern, the experiences, and the realities of prisoners of war. We should also credit the publisher for producing a fine volume. The subject and the individuals still suffer silent criticism in comparison to glorious death, but Durand describes the realities well. His study merits attention, study, and emulation.

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RAINER HUDEMANN. *Sozialpolitik im deutschen Südwesten zwischen Tradition und Neuordnung 1945–1953: Sozialversicherung und Kriegsopferversorgung im Rahmen französischer Besatzungspolitik*. (Veröffentlichungen der Kommission des Landtages für die Geschichte des Landes Rheinland-Pfalz, number 10.) Mainz: V. Hase and Koehler. 1988. Pp. xv, 616.

With the fortieth anniversary of the founding of the Federal Republic, historians of the era find themselves awash in a sea of articles, monographs, and popular histories covering the postwar occupation of Germany and the republic's birth. Although the release of archival materials located in the Bundesrepublik, Washington, D.C., and London has provided a detailed and changing view of developments in the Anglo-American Bizone, the image of the French zone remains comparatively hazy and static. The popular impressions formed at the time—that French rule was harsh, exploitative, and unenlightened—continue to color accounts of French policy in postwar Germany. Rainer Hudemann's solidly researched study, based on French

military government records in Colmar as well as on numerous German sources, seeks to modify this view. He argues that French policy was more complex than generally portrayed and, at least in the area of social policy, more generous and progressive than assumed.

In his first chapter, Hudemann discusses the economic dislocations of the postwar period and the ways in which the experience of the ubiquitous black market affected the lives and perceptions of Germans. He correctly notes that the economic difficulties were in large part the bill paid for the unsound economic policies of the Third Reich. The fiscal disarray and shortage of goods, especially foodstuffs, that marked the early years of the occupation were exacerbated, but not caused, by Allied policies. Yet most Germans out of ignorance, both genuine and willful, refused to accept this fact and instead blamed their hardships on the policies of the occupiers. Memories of the post-World War I occupation, serious structural problems in the French zone, and the undoubtedly severe consequences of some French economic policies combined to fix this totally negative image in the collective consciousness.

As Hudemann points out in his second chapter, however, French policy was neither as simple nor as unremittingly harsh as it is generally portrayed. From the start French actions were plagued with contradictions: between a policy calling for rigid political decentralization and the need in other, nonpolitical areas to pursue centralizing policies; between the goal of economic exploitation and the need for long-term security. The widespread view that the French were unalterably opposed to efforts to unite the German economy was, according to Hudemann, based on a misunderstanding. The French in fact favored a unified German economy: their zone was artificially constructed and economically weak; its integration into a larger and more dynamic economic structure would have enhanced the economic bounty that could be reaped. At the same time, however, they were adamantly opposed to the creation of any centralized German organs, which in their view was the direction in which their Anglo-American allies were headed. Others, including the American military governor, General Clay, failed to realize the distinction being made, and the view of unwarranted French resistance has become embedded in the literature.

In the area of social policy, the French not only adhered more closely to the general policies of the Allied Control Council than the Anglo-Americans but also devised programs that were more advanced and generous than those introduced in the Bizone. This, Hudemann contends, refutes the blanket charge that the French pursued an obstructionist policy in the Allied Control Council and disproves the continuing perception that French occupation policy was uniformly vindictive and harsh. In five lengthy chapters, he painstakingly traces the construction of social security, health and disability insurance, and war disability benefit programs in the French zone. Reform impulses

from within France, the idealistic convictions of French social officers, long-term political considerations, and structural constraints in Germany all combined to shape French policy. Aware of the need to win the goodwill of Germans on their border, the French implemented social policies that were more accommodating, innovative, and generous than those of the other allies. In many cases the later legislation of the Bundesrepublik represented a substantial step back. Yet, given the strength in the German collective memory of the image of universal oppression under the French, this fact has remained largely unrecognized.

This study is one of those rare books that provides more than the title promises. Although the work is detailed, at times overly so, the author never loses sight of the larger issues, and his account provides many illuminating insights into a previously unexplored area of postwar German history. Scholars interested in the period will consult this book with profit.

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WOLFGANG SCHOLLWER. *Potsdamer Tagebuch 1948–1950: Liberale Politik unter sowjetischer Besatzung*. Edited by MONIKA FASSBENDER. (Biographische Quellen zur deutschen Geschichte nach 1945, number 6.) Munich: R. Oldenbourg. 1988. Pp. 287. DM 58.

One noteworthy oddity in the German Democratic Republic (GDR) is the persistent use of the "bloc coalition" style of politics that began in the occupational period and exists today. It would be ironic if Mikhail Gorbachev's reforms end up making the coalition style a move toward some pluralism in the GDR. As Wolfgang Schollwer clearly indicates in his diary, from 1948 to 1950, the Soviets had no intention of allowing any form of pluralism to develop in their zone.

Schollwer came from a conservative middle-class family that believed in the monarchy, Prussia, and God. World War II changed all of that. He served at the front and returned to Soviet-occupied Germany and a new world. Eager to follow his father's profession of medicine, the young veteran was blocked from medical training because of his background, and he drifted into politics. Starting as a minor official in the Liberal Democratic party, he worked his way up to secretary of the state of Brandenburg and the party's expert on youth affairs. Schollwer's diary is an account, at the lower level of Sovietization, of the political parties and life in the zone during this pivotal period. He was not a theorist but a practical party functionary. What idealism he possessed evaporated quickly during the cold war. He had no illusions about the future of a united Germany given the West's growing anticommunism and the East's insistence on a Soviet Germany.

What is most striking and significant in his account is how the Soviets molded the people and parties to their own ends. Pressure, arrests, persuasion, defections,

and propaganda turned opponents into compromisers and then supporters of the regime. Everything and everybody was politicized even down to the mundane task of distributing a defector's household effects. The process was an education for the young liberal from Potsdam. He fought for his beliefs, but his sharp criticism of Sovietization soon brought him under the suspicion of the Soviet authorities. He handled it well until, stripped of his offices and under the threat of arrest, he defected to West Berlin in August 1950. He later became an important official in the Free Democratic party and the West German foreign office where he was instrumental in developing the Ostpolitik of the détente period.

Schollwer's diary will be most useful to specialists of the postwar period. The extensive footnotes explaining much of the history of the era along with selected documents and short biographies of many of the participants in the events described enhance the book's utility but limit its appeal to general readers. The work has all of the hallmarks of good German scholarship. It is an important contribution to our historical understanding of the process of Sovietization in East Germany.

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VOLKER R. BERGHahn. *The Americanisation of West German Industry, 1945–1973*. New York: Cambridge University Press. 1986. Pp. viii, 344. \$34.50.

Comprehension of a historical period often begins as that period ends. Thus, Volker R. Berghahn's illuminating analysis of the character and dynamics of the Americanization of German industry appears as the American star shines less brightly.

Berghahn holds that "Americanization" was, by and large, good for the economy, polity, and culture of West Germany, and that perspective produces both the strengths and the shortcomings of his book. Although it seems to me that the content of Americanization is never adequately elaborated, its tenets certainly included an open economy and open society with low levels of class antagonism and a strong democratic consensus. In this sense, Berghahn's position is consonant with an otherwise declining West German philo-Americanism voiced over the years by figures as diverse as Ralf Dahrendorf, Jürgen Habermas, and Richard Löwenthal.

After 1945 it was America's influence that weaned German industry away from its earlier autarkic and bloc conceptions. What emerged over the next thirty years was a highly concentrated industrial economy but one committed to free trade, both within and beyond the Common Market. Employing a sectoral model of industrial camps quite like mine, Berghahn finds a slow but steady growth in democratic sentiment within the industrial elites, along with a persistent toughness and negativism toward the trade unions and those politi-

cians, such as Willy Brandt, who appeared to stand with them.

Radical plans for socialization, or even "merely" thoroughgoing de-Nazification, were quickly abandoned after 1945. The American project became an altogether different one. West Germany was meant to be the "motor" for European production within the open "American dominated world economic system" (p. 149). One result Berghahn documents was a remarkable continuity of industrial leaders. Many men whose records of complicity in Nazism might have disqualified them from leadership found bright futures, especially as American business leaders often looked out for their German colleagues and friends. Berghahn demonstrates how the cold war, Korea, the export of accumulated American capital, and the winding down of the New Deal all contributed to the slow and arduous pace of reform in the Federal Republic. The continuance of patriarchal traditions and styles, business elitism and the cult of economic leadership groups (*Führungsverbände*), and a twenty-year-long exclusion of labor from critical decision making were both symptoms and causes.

Given the progressive meaning attached to the concept of "Americanization," but the actual conservatism of American policy, one might fairly come to the ironic conclusion that America itself was responsible for the slowness of West Germany's Americanization. This is especially true for the industrial economy when one considers the weakening of an initially strong labor movement. Liberal capitalism was a self-serving but, Berghahn reminds us, also an earnest proposition for the Americans and those Germans, such as Ludwig Erhard, who took it seriously. Thus, through the early 1950s, even American cold warriors showed "concern for openness and competition and aversion to cartels and the European bloc tradition" (p. 139). In the end, West Germany's transition from cartels to oligopolies and from autarky to free trade was facilitated by American leadership and by holding down labor's standard of living.

Berghahn's portrayal of the relations between West Germany's industrial elites and its trade unions runs counter to the rosy view so common in the literature. Whether or not it was in accord with "Americanization," the prevailing fraction of industrialists adopted and stuck to a hard line throughout the 1950s and beyond. The labor movement was marginalized, despite its relative quiescence after 1951 and a nearly thirty-year-long absence of significant strikes. The tripartite corporatism—perhaps progressive but certainly not American—for which West Germany has become known in the last twenty years was not evident in that country's first two decades. Even after the unions and the Social Democratic party abandoned their Marxist pretensions, they remained unwelcome in the corridors of economic power and decision making. Berghahn argues instead for a much more traditional bipartism in which the industrial elite enjoyed access to and influence over critical state institutions.

Only after the shift in the balance of power within industry was substantive cooperation with labor possible. Thus, Berghahn associates the shift to tripartism with the rise of the automobile and export industries and of Karl Schiller in the mid-1960s. Whether subsequent developments, especially the expansion of the welfare state, *Ostpolitik*, and Brandt's domestic reforms, are best understood in the context of "Americanization" may be doubted. America was reticent toward all three. And by the late 1960s, the official American image had lost much of its lustre and force in West Germany, unless one chooses to subsume even the civil rights and anti-authoritarian movements under the rubric of "Americanization."

"Concerted action," extension of industrial codetermination, and a greater voice for unions in government policy belong to the successes of social democracy after 1968 and to Germany's bureaucratic tradition. Comparison with the United States suggests to me that they have little to do with either free trade or American culture. For Berghahn, however, the minority of more forward-looking German industrialists reconciled themselves to these developments as a means to integrate youth and workers into the private economy. This represented a kind of attenuated Americanization of attitudes or at least a taming of industrialist hubris.

Nonetheless, Berghahn finds that the employers' posture in the codetermination debates around 1970 was not much more forthcoming than it had been in 1950. The dubious efforts by industrialists to topple the Brandt government also bespeak a continued confidence in the privileged status of capitalist interests. That this was either peculiarly German or American may be doubted. But it was certainly not new.

In fact, despite Berghahn's emphasis on generational change, what is striking in his presentation is the similarity in industrialist language (and family names) between 1930 and 1970! Thus, men such as Hermann Reusch, Rudolf Schlenker, and Jürgen Heinrichsbauer—each the son of a leading reactionary figure in the industrial milieu of 1930–33—railed against "economic democracy" and urged "no compromise" (p. 312) with the socialist threat. The unions and the Left had "a clear three-stage plan for the abolition of the market economy by way of a communalization of the land, confiscatory taxation and a socialisation of the means of production" (p. 322). In critical respects Americanization did not alter very much. West German industry's commitment to democratic public institutions doubtlessly grew in the generation after 1945. Despite Berghahn's rich and detailed narrative, it is not clear how much credit for that we Americans or our open world economy ought to take. Perhaps our most significant contribution lay in helping to defeat fascism. At the very least, we may want to consider the possibility that it was the Americanization of other social groups in West Germany that contributed to building West Germany's liberal democratic character.

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DANIELE MONTANARI. *Disciplinamento in terra veneta: La diocesi di Brescia nella seconda metà del XVI secolo*. (Annali dell'Istituto storico italo-germanico, number 8.) Bologna: Mulino. 1987. Pp. 351. L. 33,000.

This book presents the thesis that one of the most striking and specific characteristics of post-Tridentine Catholicism was a new concept of the parish. Reforming bishops increasingly envisioned the parish as the basis and center of the religious and socioeconomic life of the people. Daniele Montanari has minutely examined the diocese of Brescia under its bishop, Domenico Bollani, from 1559 to 1579, in order to show how this ideal was implemented. Montanari argues that the changes brought about in the sixteenth century were illustrative not only of the Counter Reformation but also of a wider attempt in early modern Europe to bring a new spirit of order and discipline to ecclesiastical structures. Such order was thought to benefit both church and state. Brescia is an unusually interesting case for study because it was politically subject to Venice and ecclesiastically dependent on Milan. Diocesan reforms had to proceed in ways acceptable to both secular and ecclesiastical superiors.

Bollani, we are told, embraced with conviction the ethos of the Venetian ruling class and did not become a carbon copy of his archbishop, Carlo Borromeo. Unfortunately, there is little subsequent mention of the role played by either Borromeo or the Venetian government in the formulation and implementation of Bollani's reform measures.

The discussion of these reform measures constitutes the heart of Montanari's book. Beginning with a description of the diocese and its geographical division into plain, foothills, and mountainous areas, the author next looks at the structure of benefices and sources of material support for priests and churches. Reform measures directed at the secular clergy, pastoral regulations for the laity, guidance and supervision of their religious and charitable activities and of the institutions associated with them are the main subjects examined.

The records of the diocese of Brescia do not reveal surprises. Montanari's findings support conclusions of recent works on the late medieval church and the growing number of studies dealing with the varieties of popular devotion. One of the strong points of his book is the integration of the evidence from Brescia into the wider picture of the church in Western Europe. In Brescia, too, we find the serious problem of clerical ignorance, the enormous gap between holders of rich benefices and the clerical proletariat, nepotism, and inequities in the allocation of diocesan resources. Bollani's directives dealt with all of these issues. The special focus of his reforms was the attempt to raise the educational level of priests with concomitant improvement of preaching and catechetical instruction. As one might expect, conflicts between the bishop's desires and long-established patterns of popular devotion and customs were inevitable. Especially in the mountain parishes, age-old usages accompanying birth, marriage,

and death did not change quickly in response to prescriptive norms issued by a remote urban bishop. Bollani and his supporters were more successful in impressing a new spirit of order on confraternities, especially that of the Most Holy Sacrament, so typical of the Counter Reformation, and on lay institutions dispensing charity to the poor.

Montanari's book offers extensive evidence for the gradual bureaucratic streamlining of ecclesiastical organization in the wake of the Council of Trent. But some questions remain unanswered. How typical does the author consider Brescia to have been? It was, according to him, unusual in its ties to both Milan and Venice. But the effect of those ties on the process of reform remains vague. How crucial was the support of the secular government in successes Bollani actually achieved? A thoughtful conclusion instead of the abrupt ending would have been helpful to the reader and a definite improvement of the book.

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VOLKER HUNECKE. *Die Findelkinder von Mailand: Kindaussetzung und aussetzende Eltern vom 17. bis zum 19. Jahrhundert*. (Industrielle Welt; Schriftenreihe des Arbeitskreises für moderne Sozialgeschichte, number 44.) Stuttgart: Klett-Cotta. 1987. Pp. 287.

Volker Hunecke has made a striking contribution to our understanding of parental behavior and survival strategies among the European urban lower classes during the seventeenth, eighteenth, and nineteenth centuries. In his study of child abandonment, Hunecke has concentrated on the history of the foundling hospital of Milan. The hospital was founded in 1458 and reorganized during the reign of Maria Theresa, who also ordered that it should be supported by public funds. Hunecke has based his research on the very complete archives of this hospital. He has also used the baptismal and census data compiled by church and municipal authorities to trace the life histories of abandoned children and their parents. Because the data are more complete for the nineteenth century than for previous centuries, he has selected one representative year, 1842, for a thorough reconstruction of biographical data on the families that abandoned children at the foundling hospital. Such research into family backgrounds was made possible by the surprising fact that the majority of children left in the revolving cradle at the door of the hospital were legitimate, had been baptized, and were provided by their parents with some kind of identification. Hunecke, thus, has centered his attention on what he has identified as a neglected aspect of the history of abandoned children—the circumstances that motivated their parents to abandon them. The parents are very thoroughly described through analyses of their employment, marital, and reproductive patterns and through summaries of individual life stories. Hunecke has provided an

appendix giving extensive quantitative data on their ages, the size of their households, and other demographic characteristics.

Hunecke's painstaking and thorough research has yielded some results that challenge many historical views of parent-child relationships among the urban poor during this period. In their analyses of parental behavior from the early modern period to the early nineteenth century, historians such as William L. Langer, Elisabeth Badinter, and Edward Shorter have characterized the widespread tendency of parents to consign their children to hired wet nurses or to foundling homes as a sign of indifference or even a disguised form of infanticide. Those views have been disputed by several studies of child abandonment, such as Rachel Fuchs's *Abandoned Children: Foundlings and Child Welfare in Nineteenth-Century Paris* (1984). Like Fuchs, Hunecke sees the abandonment of children to foundling hospitals not as a sign of parental indifference but as a survival strategy during times of economic or personal crisis. Other aspects of the stories told by Hunecke and Fuchs differ strikingly, however. Both the Parisian and Milanese hospitals were set up in order to "rescue" illegitimate children from their mothers, who were assumed to be unfit parents. But although the Parisian foundling hospitals received chiefly illegitimate children, the majority of the babies deposited in the revolving cradle in the door of the Milan hospital were born of married parents. Often a Milanese family would consign several, or even all, of its children at birth to the hospital. Moreover, unlike the Parisian mothers, most of whom probably parted with their children forever, the majority of the parents in Milan reclaimed their children within a few years. The contrast between these two studies suggests fascinating possibilities for comparative research. What institutional, economic, demographic, or cultural patterns account for the differences in behavior?

Hunecke's study also sheds light on another much-discussed question: the relationship between welfare institutions and the people they serve. The widespread use of the hospital by married parents rather than by the unwed mothers for whose use it was intended provides an example of the ineffectiveness of many attempts to use philanthropy to impose middle-class moral standards on the poor. In 1868 the hospital tried to end such "abuse," which had caused much indignation among its administrators, by closing the revolving cradle that had permitted anonymous abandonment. Was that decision influenced by the similar action of the Paris hospital in 1862?

Hunecke has produced a thorough and innovating study that provides insight into the behavior of a social group that has hitherto received little attention from historians. The personal stories of the parents are often profoundly moving. It is unfortunate that this book will appear more to specialists in the field than to general readers.

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JOHN A. DAVIS. *Conflict and Control: Law and Order in Nineteenth-Century Italy*. Atlantic Highlands, N.J.: Humanities. 1988. Pp. 398. \$45.00.

In this perceptive study, John A. Davis is less concerned with police procedures than with the politics of law and order, the accommodations between Italian governments and elites, and the impact on all classes as these relations shifted in a growing economy. Such adjustments generated fear of unrest and made public order a primary political consideration through the nineteenth century. The dissolution of the old order and the emergence of a new are traced through political restructuring and a century-long reorganization of agriculture. Davis believes that the crucial factors that turned many elites toward unification were the inabilities of Restoration governments to maintain order and the identification of Piedmont after 1848 with constitutional government and political liberties.

The interpretive patterns so briefly indicated reveal a kind of order in dissimilar circumstances. The attempts in the *mezzogiorno* to use collective occupations of land to reassert communal rights would have made little sense in the Upper Po Valley where intensive agriculture had already destroyed community life. Yet rural unrest helped bring propertied elites of both regions to support unification. Davis is less successful in assessing the problem of vagrancy, speculating that its apparent increase was less the result of evictions from land than of population increase and alterations of leases. He is probably right, although quantitative data are lacking. Certainly the confusion of indigence with crime in the sources because of the Napoleonic Code's definition of vagrancy as itself a legal offense is symptomatic of response to social dislocation as a police problem. Even after unification, threats to order continued to justify carelessness of constitutionality. Witness the government's overreaction in assuming danger and using force against the ambiguous millenarian movement of Davide Lazzaretti in the 1860s.

The special character of urban unrest is less adequately examined. That towns had a protected status (even after abolition of special provisioning regulations) is seen, for example, in police measures limiting immigration of the rural poor. Yet town dwellers were often far from prosperous, and urban revolts were frequent. Davis observes that urban Italy during this period has been relatively neglected by historians. The current research of Steven Hughes on Bologna will be valuable.

In an epilogue Davis views the crisis of the 1890s as an ambiguous turning point for liberal Italy as new social forces threatened the accommodation that had evolved between Italy's elites and government. Contemporaries understood, says Davis, that the abandonment of free trade in 1877 implied a choice of economic sectors to be protected and, therefore, a shift in the interests closely identified with the state. It also implied a price and wage structure damaging to the poor. Forces of change are discovered in economic

expansion, industrial consolidation, and the emergence of mass politics (both socialist and Catholic), and new attitudes symptomatic of imperialism and nationalism and possibly contributing to Fascism are identified. But exactly how these dangers generated a broad political alliance on the issue of civil rights to defeat a policy of military suppression of disorders is not clear. Certainly the obsession with law and order was changing. But protectionism, for Davis a key element in realignment, is missing from his account of the political crisis. Wisely Davis refers the full significance of the turning point to the evidence of future events. Scholars will be grateful for the insights of his study, which will help set the terms of much subsequent research.

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ALFONSO BOGGE and MODESTO SIBONA. *La vendita dell'asse ecclesiastico in Piemonte dal 1867 al 1916*. (Studi e ricerche di storia economica italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana. 1987. Pp. 1,525.

The government of newly united Italy lost little time in confiscating and selling church-owned properties not used for religious purposes. To contemporaries, the laws passed in 1866–67 were variously a sober (or desperate) effort to reduce the state's debts, a rejection of evils (mortmain and clerical dominance) from the benighted past, an unwarranted attack on religion (purchase of these properties brought excommunication), and a source of social unrest (including violence in Palermo). In the next generation, even cautious leaders such as Stefano Jacini and Sidney Sonnino denounced the procedure for allowing most of this land to fall into the hands of the rich. Historians have tended to be still more critical, finding in this episode an example of the liberal regime's inadequacies. The laws had loopholes; quick sales drove prices down, defeating the government's principal purpose; speculation and corruption were rife; a wedge was driven between peasants and government; only a ruthless bourgeoisie benefited; the opportunity for social reform or for modernizing the rural economy was missed.

Until now, however, these sales have never been systematically studied, and the findings in this study of Piedmont are striking. As Alfonso Bogge and Modesto Sibona note, the region was atypical in a number of ways. The thorough application of earlier Napoleonic measures and the law of 1855, which confiscated the property of unauthorized congregations, had left the church in Piedmont with somewhat less property to be confiscated; government there was more efficient than in most of Italy and the economy more developed. Still, the authors of this extraordinary work consistently compare their findings with what is known for the rest of Italy and suggest that work like theirs on other

regions might come up with similar results. Such new studies are not likely soon. This one took nearly twenty years. The details in this volume can be overwhelming; half of its fifteen hundred pages list the more than fifteen thousand sales of church property in Piedmont between 1866 and World War I (with brief descriptions and names of purchasers). Numbers nevertheless have a wonderful way of putting things in perspective. The properties sold in Piedmont constituted about one-half of the church's wealth and two-thirds of its individual holdings, a somewhat smaller proportion than in other regions. Nationally, the revenue from these sales (primarily through the bonds they supported) equaled just under 7 percent of state expenditures for the period 1861–70. The authors' most surprising conclusions, however, have to do with the sales themselves. The government's system for rapid appraisal of property was sensitive to local conditions and worked remarkably well; administrative costs were relatively modest. Some 78 percent of the land was quickly sold at prices close to market value, most at open auctions. Sales took place in hundreds of localities, most often administrative centers, which provided wide access. Even as auctions gave way to written bids and then private sales, there is little evidence of corruption, and, on the whole, officials of all sorts, who were not frequent or major purchasers, seem not to have benefited unduly.

Property tended to be sold in sizes similar to other holdings in the immediate area (this was not a program for social reform). For that very reason, lots varied enormously in size and value, but 90 percent of the lots were small enough and cheap enough (helped by government discounts) to be within the means of a majority of the population (the authors compare deposits in rural savings accounts). Although it was possible to buy on time, most purchasers were peasant owners rounding out their properties, and they chose to pay cash. Only about 10 percent of the sales went to speculators, and most of these (often Jews) were locally welcomed for then selling in still-smaller lots.

Socially and economically, then, the results were not dramatic and certainly not disruptive. Although the authors believe that a significant redistribution took place, they see it as part of a long-term process. The church had held much more property in the plains than in the mountains, and the new owners tended to use this land more intensively and profitably, where market forces encouraged the change. When Catholics petitioned for removal of the religious sanctions against buyers of church property (often citing their purchase through an intermediary, especially a Jew), owners of small lots were more readily pardoned than their richer neighbors.

In sum, this weighty book presents a radical revision of standard views. Despite the 239 tables and thick array of analyses by crop, region, and buyer, analyses far richer and more complex than can be indicated here, there remains room for some dissent. Many of these conclusions rest on available samples, and specialists will dispute their representativeness (the au-

thors themselves urge caution). The possibilities of sophisticated statistical analysis are largely eschewed in favor of percentages sometimes bewildering in their frequency. Large properties did make up the majority of the land sold, which supports suspicions of social favoritism. Those convinced that the state lacked competence, that class interests dominated, that corruption was endemic in rural Italy, and that unification and capital were disruptive will insist on treating Piedmont as far more of an exception than the authors allow. No one, however, can approach these issues with the ideological certainty that has marked the discussion until now.

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FERNANDO VENTURINI. *Un "sindacato" di giudici da Giolitti a Mussolini: L'Associazione Generale fra i Magistrati Italiani, 1909–1926*. (Collana di Storia Contemporanea.) Bologna: Mulino. 1987. Pp. 312. L. 30,000.

The extralegal violence and "political justice" associated with the Fascist rise to power and the consolidation of an authoritarian state in Italy have naturally drawn scholarly attention to the problems and weaknesses of the Italian judicial system in the early twentieth century. Above all, a number of works have appeared since 1970 that explore the complex relationship between the judiciary and political power. Most recently, Pietro Saraceno has directed a collaborative project exploring the prosopography of Italian magistrates from unification to the Fascist regime. Out of this larger project has emerged Fernando Venturini's meticulously researched institutional history of the magistrates' efforts to develop their own union organization, the Associazione Generale fra i Magistrati Italiani (AGMI). Venturini has had to overcome a number of obstacles to study the AGMI. Because the association's records were destroyed in the mid-1920s, he has thus had to rely on the AGMI's official publications and on ministerial dossiers to reconstruct the story. The results are predictably mixed.

The circumstances that provided the impulse to organization, in Venturini's account, were complex and contradictory. In part they reflected a new collective consciousness among Italian magistrates who desired a more direct role in the elaboration of judicial policy. More immediately, they reflected a reaction against Giovanni Giolitti, Italy's foremost prewar statesman, whose tolerance of social and political conflict many magistrates viewed as a threat to the authority and prestige of state institutions. At a more mundane level, demands for economic amelioration and career advancement were of great importance, especially among junior magistrates. Such concerns did not easily mesh with the aims of the top magistrates who saw the AGMI as a means to pressure governmental authorities and reinforce traditional hierarchical relations within the judiciary.

Until the end of World War I, the AGMI was plagued by a variety of problems. Coherent policy making was hampered by a tendency to oscillate between the idealistic promotion of the leaders' reform objectives and the pragmatic advancement of the members' economic and professional interests. Similarly, in its efforts to exert influence, the AGMI oscillated between reliance on the support of highly placed political figures and use of propaganda to mobilize public opinion. The relative isolation of the magistrates ended in 1919, but only briefly, when the AGMI sought "productivist" alliances with other labor organizations. On the magistrates' role in the rise of Fascism, Venturini plays down the ideological sympathies of those who were organized, emphasizing instead an uncertainty and disorientation that led them to misjudge Benito Mussolini's movement. Not surprisingly, the triumph of Fascism marked the beginning of the end for the AGMI, which was soon deprived of any influence over judicial policy, while its leaders were removed from public life.

These broad contours of institutional evolution do not easily emerge from a study that is decidedly for specialists. Venturini provides little background information to help the reader understand the context in which the AGMI operated. More seriously, with few middle-range generalizations, his minutely detailed treatment tends to confuse rather than illuminate. Such difficulties arise from Venturini's tendency to analyze painstakingly his documentary sources without integrating them effectively into his narrative. This shortcoming is unfortunate because it obscures the valuable insights he does have into the brief and troubled history of the organized magistrates in liberal Italy.

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STEPHEN HELLMAN. *Italian Communism in Transition: The Rise and Fall of the Historic Compromise in Turin, 1975–1980*. New York: Oxford University Press. 1988. Pp. x, 274. \$34.95.

The Italian Communist party has long bedeviled observers of Italian politics. By the middle of the 1970s, the results of the national and local elections placed the Partito Comunista Italiano (PCI) on the doorstep of power, and party leaders were proposing the *compromesso storico* to facilitate a governing alliance between the PCI and the struggling Christian Democratic party. Political scientists organized symposia and constructed theories to explain the inevitability of the party's ascension to power. Events have denied these expectations. The Communists have not yet assumed control of Italy and are not likely to do so in the near future. In Stephen Hellman's words, "Instead of riding the wave of the 1970s, the PCI was flattened by it" (p. 218). This

volume helps to explain the PCI's failure and to solve the puzzles that have perplexed analysts of this party.

Hellmann's study is a model of carefully crafted scholarship that draws on an extensive and intensive familiarity with the object of study, the Communist party in Turin. The author eschews technical modes of analysis. His fieldwork uses detailed knowledge of local activists, while avoiding the self-conscious methodology of many anthropologists, and he denies the utility of general theories of parties and organizations. This is a classic case study. Especially because of the detailed manner of the exposition, the publisher's decision to use a very small font size and to install narrow margins has done the author and his readers a disservice. The mode of analysis and the text combine to make the volume especially dense. The reader should persevere, because Hellman creates his argument by slowly and precisely piecing together the strands that have composed the communist movement in Turin during the past decade.

At the heart of the story lies the *partito nuovo*, the structure created by Palmiro Togliatti for the PCI at the end of World War II. For many decades a source of political strength, the organization merges the mass characteristics of the classic working-class party and the closed, conspiratorial, and professional structure that marks the descendants of the Third International. By the middle of the 1970s, the communist movement in Turin faced several challenges. Long a militant opposition party, structured around the industrial workers in Italy's most heavily mechanized region, the Turinese party controlled local governments. Changes in the structure of industrial Italy weakened the power of workers long associated with PCI in the factories of Turin, increasing the presence of technical and white-collar workers and allowing industrial capital to roll back gains that the unions had obtained a decade earlier. Successes in parliamentary elections induced Enrico Berlinguer and other national leaders to seek cabinet ministries in Rome just as terrorists attacked the symbols and leaders of the Italian state. Hellman shows how the effort to reach power in Rome by striking a bargain with the Christian Democrats drew the PCI away from its traditional sources of strength, forced them to attack left-wing militants, induced them to miss the real significance of new social movements, and even led to conflict between the party and the left-wing unions of the Italian General Confederation of Workers. Hellman details how the *partito nuovo* now inhibits the communist movement's ability to adapt to a new Italy. As a result, the study depicts the limits that even the most organized political parties face in controlling their social base in a democratic society.

ALAN S. ZUCKERMAN
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KAROL MODZELEWSKI. *Chłopi w monarchii wczesnopiastowskiej* [Peasants in the Early Piast Monarchy]. Sum-

mary in French. Wrocław: Ossolineum. 1987. Pp. 295. 700 Zł.

Few problems in Polish historiography have been as controversial as the question of the status of the peasantry in the early Middle Ages. Even before the emergence of post-World War II scholarship, the issue had long been debated. Beginning in the 1950s, however, a new round in the discussion was opened by a series of publications, some of the most important of which came from the pen of Karol Buczek. His picture of early Polish society under what had come to be known as "ducal rule" (*ius ducale*) was challenged directly by the work of Karol Modzelewski, especially in his 1975 study on the economic organization of the Piast state from the tenth to the thirteenth centuries.

The book under review here represents Modzelewski's most recent contribution in this continuing debate. (In the West, many of the interpretations argued by Modzelewski have been challenged by the German historian Oskar Kossmann, who, while defending some of Buczek's views, nevertheless presents his own independent position. The work of the American scholar Piotr Gorecki represents yet another important approach to these issues.) The publication of Modzelewski's book as the first of a multivolume series on the history of the peasantry in Polish society, sponsored by the Institute of History in the Polish Academy, suggests the degree to which Modzelewski's views have begun to be regarded as normative. (Six additional volumes in the series are projected to carry the history of the peasantry into the post-1945 era.)

Modzelewski begins by discussing the status of the free population prior to the rise of ducal (and eventually royal) rule. In his second chapter, he analyzes the impositions levied on the peasantry by the early Piast monarchy, concentrating particularly on exactions in kind and on service obligations. The third chapter treats the categories of the peasant population and has some particularly insightful comments about those on the margins of rural society and about knightly rights over otherwise free peasants. A fourth chapter discusses the nature of rule over the peasantry and the organization of the ruling classes, especially the nature and degree of wealth commanded by these strata. The fifth chapter is devoted to an analysis of the relation between the rights of the ruler and those of the older rural settlements (the Polish *opole* or, in Latin, *vicinia*). A penultimate chapter discusses the changes in social organization wrought by settlement from abroad (the period of "colonization") and the acquisition of various immunities, especially by the church. Developments in the twelfth and thirteenth centuries eventually overturned the older system of *ius ducale* but did little to alter the reality of peasant life. Those who received immunities from ducal rule nevertheless preserved control over the non-free population, the *ascripticii*. Modzelewski's final chapter is devoted to an analysis of the role and status the peasantry had within the religious and state community. His comments about the

integration of the peasantry into a society that held the promise of a national, rather than a regional, base are especially provocative.

Not all will agree with Modzelewski's interpretations—either in Poland or in the West. The data are fragmentary and ambiguous, and a different theoretical framework from the one within which he works can lead to substantially different conclusions. His arguments are, however, subtle and stimulating and are sure to give pause to any who consider the nature of early Polish peasant society.

PAUL W. KNOLL

University of Southern California

RUDOLF JAWORSKI. *Handel und Gewerbe im Nationalitätenkampf: Studien zur Wirtschaftsgesinnung der Polen in der Provinz Posen (1871–1914)*. (Kritische Studien zur Geschichtswissenschaft, number 70.) Göttingen: Vandenhoeck and Ruprecht. 1986. Pp. 205. DM 38.

The Poles in the German province of Poznań in the late nineteenth century were living in one of the most modern and most dynamic countries in Europe. Germany was rapidly industrializing; it was a leading military power; its science and culture, schools and universities, were admired and emulated not only in Europe but also in other parts of the world (for example, Japan).

In comparison with the Germans, the Poles were weak and underdeveloped. What were they to do in those circumstances if they wanted to survive as a distinct nation? They chose to develop their own program of modernity that would be able to compete with German culture, German civilization, the German state. They charted out a Polish road to capitalism—a capitalism not simply in one country but, because Poland was divided between three great powers, in one part of one country.

It was by no means an easy task. Unlike the Germans, the Poles were still an overwhelmingly rural, agrarian society. The dominant element of the Polish community was the nobility, a class not known for its commitment to "modernization," industry, trade, or city life in general. And yet the Poles created a Polish capitalist ethic as it were, in which economic development, the building up of Polish commercial enterprises, banks, and saving societies, was treated as a national activity, whose aim was to preserve and develop a nation under alien rule. One might say that they even managed to erect, within one state, within a single province, a customs boundary to separate their businesses from their German competitors. (In this they were following a path first traversed by the Czechs.) It was no less effective for being "imagined," in the sense in which, according to Benedict Anderson, nations are "imagined communities."

Rudolf Jaworski does not cover the entire story: his book is not a history of the Polish effort, nor is it an account of how the Germans responded to Polish

actions. Instead, he presents the "Polish ideology," that is, the arguments the Poles advanced to define their goals and the methods they adopted to realize them. His main sources are Polish literature and the press, where these economic problems were treated in a broader national frame.

Jaworski has written an important book. It is a very valuable addition to literature and can be read especially usefully along with the earlier fine book by Stanislaus A. Blejwas, *Realism in Polish Politics* (1984), in which Blejwas covers Russia's part of Poland. Jaworski's book is a revealing case study of that complex interplay between material interests and ideas, tradition and change, nationalism and capitalism.

ROMAN SZPORLUK
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JERZY KOZŁOWSKI. *Rozwój organizacji społeczno-narodowych wychodźstwa polskiego w Niemczech w latach 1870–1914* [The Development of Social Organizations among Polish Immigrants in Germany, 1870–1914]. Summary in English. (Polska Akademia Nauk; Komitet Badania Polonii; Biblioteka Polonijna, number 18.) Wrocław: Zakład Narodowy im. Ossolińskich. 1987. Pp. 288. 420 Zł.

Apart from Jerzy Kozłowski's good intentions and industriousness, there is little to commend in this book. Yet it is worthy of brief comment because it exemplifies one of the crises besetting not only establishment historiography in present-day People's Poland but also Polish historical consciousness itself.

Kozłowski writes of the emergence and growth of "social-national organizations" among the increasingly large number of Poles who migrated from their ethnic homelands in the Prussian east to the central and western regions of imperial Germany, above all, to Berlin and to Rhineland-Westphalia's booming industrial towns of the Ruhr district. By 1914, nearly 20 percent of the empire's Polish-speaking citizens lived in the German heartland: one-half million in the Ruhr, eighty thousand in Berlin, and the rest scattered from Saxony to Hamburg. They were humble people, born of the eastern peasantry and working class and aspiring to the higher standards of living prevailing among German laborers, coal miners, and foundry workers whose occupational ranks they joined. They were also, to a high degree, fervent Polish Catholics, and no other issue in the emigrant communities exercised them more than the Prussian government's (and the German Catholic hierarchy's) refusal, especially after the early 1890s, to allow Polish-born priests to minister to their spiritual needs.

Parish-based "Polish-Catholic societies" mushroomed throughout the emigrant communities, pressing the authorities for Polish priests and Polish-language masses. From these ranks emerged plebeian activists who, joined by middle-class nationalist organizers from the eastern provinces, spun an impressive

web of artisans' and petty merchants' clubs, gymnastic societies, reading circles, choral and dramatic troupes, patriotic women's organizations, an increasingly formidable Polish labor union (Zjednoczenie Zawodowe Polskie, which in 1914 was sixty thousand strong in the Ruhr alone), and, finally, political associations whose vociferous support in Reichstag elections for hopelessly outnumbered Polish candidates spread the message of a radical populist Polish nationalism.

Unfortunately for Kozłowski, this story, especially as it played out in the Ruhr district, has already been ably told, notably by the Polish historian Krystyna Murzynowska (*Polskie wychodźstwo zarobkowe w Zagłębiu Ruhry w latach 1880–1914* [1972]) and, in the German literature, by Christoph Klessmann (*Polnische Bergarbeiter im Ruhrgebiet 1870–1945* [1978]). What Kozłowski adds about Berlin and the other emigrant colonies opens no new perspectives.

Still, his book has one striking quality: its relentless celebration, free of any Marxist-Leninist inspiration whatsoever, of Polish-Catholic nationalism or, as he quaintly puts it, of "the consolidation [among the emigrants] of national consciousness and the encouragement [through Polish organizations] to abide in Polishness" (p. 105). Kozłowski naively regards every manifestation of Polish patriotism as a blow for national independence, for release from the captivity of foreign occupation ("niewoli zaborczej" [p. 275]). He writes with approval that the National Democratic movement, whose intolerant chauvinism blighted interwar Poland, trained its legions of young gymnasts to be "burning patriots, Pan-Polish and anti-German" (p. 197). Similarly, the anti-Semitism of the Polish populism of the period goes unmentioned. Where the Prussian police saw "Great-Polish agitation" in the rhetoric and activities of the most harmless of Polish organizations, Kozłowski takes them at their word, extolling the national-Polish ("narodowopolskie") implications of emigrant behavior, even when, as at the end of one Polish assembly in 1891, the kaiser's name was cheered (p. 144).

Imperial Germany's Polish Socialist party (PPS), feeble in numbers though it was, also wins Kozłowski's praise, not because it tried to wage the class struggle on the Polish workers' behalf but because, despite its divisive tactics, it, too, struggled for Polish independence. Such is the discredit into which Marxist-Leninist analysis and even the very idea of socialism—both prominent in Murzynowska's book—have fallen in the eyes of many contemporary Poles, including officially sponsored historians. But an uncritical evocation of national solidarity in the past will yield no deeper understanding of the dynamics of Polish history in modern times than Marxist-Leninist dogmatism did.

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ANDRZEJ CHOJNOWSKI. *Piłsudczycy u władzy: Dzieje Bezpartyjnego Bloku Współpracy z Rządem* [The Piłsudskiites in Power: The History of the Nonparty Bloc for Cooperation with the Government]. Warsaw: Ossolineum. 1986. Pp. 294. 400 Zł.

When Marshal Józef Piłsudski seized control of the Polish government through a coup d'état in May 1926, he immediately confronted the dilemma of how to deal with a parliament and a government apparatus dominated by his opponents. Disinclined to abolish the system of representative democracy and resort to overtly totalitarian rule, Piłsudski opted for an approach calculated to gain supremacy in the legislature as well as in the administrative bureaucracy for his followers, who could then assure him of compliant acceptance and effective implementation of his desires. This approach allowed him to maintain the appearance of observing the principles of parliamentary democracy while effectively ruling as a dictator. Although other Polish scholars have examined the entire spectrum of political history or specific aspects of it following the coup, thus far none has focused specifically on the organization designed to institutionalize, and simultaneously legitimize, Piłsudski's methods of governing. Thus, the superb study by Andrzej Chojnowski adds a crucial missing piece to the puzzle of the *Sanacja* period in interwar Polish history.

The Bezpartyjny Blok Współpracy z Rządem (Non-party Bloc for Cooperation with the Government [BBWR]) was originally formed in early 1928 to represent the pro-Piłsudski forces in the parliamentary elections scheduled for March. Rather than being just another political party, the bloc functioned as an electoral alliance composed of small political parties and groups of social, economic, and trade union activists sharing a progovernment orientation and willing to run candidates on the BBWR list instead of on their own. Although the bloc performed below expectations in the 1928 elections, two years later it was able to gain a majority sufficient to give Piłsudski the mandate he required over an opposition decimated by the Brześć purges. Subsequently, the bloc assumed the character of a formal extraparlimentary structure that claimed to offer a qualitatively new method of organizing Poland's social and political life. In contrast with existing political parties, the bloc was noteworthy for its supposed lack of partisan bias, its origin (created by state authorities), its deliberate absence of a program, its loose organizational structure, and its lack of genuine internal political cohesion. But the bloc never realized its publicly stated, envisioned potential, because it proved impossible to arouse authentic social initiative from above through dictatorial means. Instead, the BBWR degenerated into a convenient tool for administrative authorities at all levels, particularly the lower, to enforce the regime's rule over an increasingly recalcitrant citizenry, while also offering well-paid jobs in the burgeoning state bureaucracy to friends and relatives of bloc leaders. Of course, in the final analysis,

the BBWR did, after all, fulfill its missions of dominating the parliament, monopolizing the administrative apparatus, and, on the eve of Piłsudski's death and the bloc's dissolution in 1935, pushing through a revision of the constitution in a more authoritarian version.

Chojnowski skillfully profiles this political creature, which he compares with the Patriotic Union in Spain, the Homeland Front in Austria, and the Front for National Rebirth in Romania. Commencing with a thorough analysis of the intellectual and practical origins of the bloc, he then chronicles its performance during the difficult, challenging period from 1928 to 1930 and the more successful and less stressful final years of Piłsudski's rule. The narrative is enhanced by use of an impressive base of primary sources, including archival and manuscript materials hitherto not tapped to their fullest extent, and an appendix containing selected bloc publications and speeches and pronouncements of BBWR leaders. Although the presentation of some statistical data in tabular form and the inclusion of an organizational diagram would have enhanced the work somewhat, their absence should not obscure the fact that this book should remain for the foreseeable future the definitive study of the Piłsudski regime's chief political organization.

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HILLEL J. KIEVAL. *The Making of Czech Jewry: National Conflict and Jewish Society in Bohemia, 1870–1918*. (Studies in Jewish History.) New York: Oxford University Press. 1988. Pp. viii, 279. \$29.95.

In the nineteenth and early twentieth centuries, the Jews of the Czech lands, as elsewhere in Europe, experienced complex processes of emancipation, assimilation, and the development of modern civic identities. Unique in comparison to Jews in most of the neighboring lands, Jews in Bohemia and Moravia had to choose among two competing secular national identities, Czech and German, and an emerging Jewish national loyalty. Hillel J. Kieval's study traces the rise of the Czech-Jewish national movement and the Bohemian variant of Jewish nationalism from Austria's German liberal era to the first years of the Czechoslovak republic.

Kieval considers the thought and political activities of the Czech-Jewish movement's leaders and their Prague Zionist counterparts as emblematic of a more general transition of Bohemian Jews from the earlier era of assimilation and support for Austro-German culture and political liberalism. He takes their work as signs of the Jews' "disenchantment" (p. 3) after 1870 with their previous loyalties. Well written and neatly organized, this book is the first major study in English of the Czech-Jewish movement, and, for both the Czech-Jewish leaders and the better-known Zionists, Kieval provides succinct reviews of their evolving perspectives on Jewish culture and identity through the early 1920s.

The attraction of Martin Buber's neo-Romantic notions of Jewish identity and the differences between the Prague Zionists and the members of mainstream Austrian Zionism are explained convincingly. Specialists on Czech history and Czech Jews will find little that is startling here on political and organizational developments per se, but those interested in Jewish political and intellectual history in other parts of Europe will find this book of considerable value.

There are some significant shortcomings, however. Despite efforts to generalize about all Czech Jews, Kieval addresses Jewish experience in Bohemia almost exclusively. The patterns among Moravian Jews do not fit the author's thesis as well: the Czech-German conflict developed somewhat later and less intensely there, and the Moravian Jews' German loyalties were even stronger and more persistent than were those of the Jews in Bohemia. Even in his examination of Jews in Bohemia, Kieval tends to treat traditions of German identification as a residual category and to write off prematurely, by at least a decade or two, the old habits of German acculturation and organizational affiliation, despite such evidence as the continuing Jewish enrollments in German secondary and higher education. Part of the difficulty here is that the study, although offered as a social and cultural history, tends to focus on the attitudes and some of the organizational activities of the intellectual vanguard, whose members were indeed outspoken in their rejection of the old German liberal synthesis. Discussions of popular responses to or support for the Czech-Jewish and Zionist movements are brief and sketchy. The intellectually interesting Zionist student organizations Bar Kochba and Theodor Herzl are examined, for example, but the earlier and more popular Bohemian branches of the Austrian Jewish Volksverein Zion are barely mentioned.

By the author's own account, neither the members of the Czech-Jewish movement nor the Prague Zionists succeeded in attracting any significant organized following among the Jewish population at large before 1918. The reader can only wonder what was actually happening to the values and civic identities of the Bohemian, Moravian, and Silesian Jews in general. More could be learned about support for the Czech-Jewish and Zionist organizations from the police reports, the papers of the Bohemian and Moravian governors' offices, and other manuscript materials in the Czechoslovak state archives. Curiously, Kieval's notes cite a considerable body of rare printed materials from the libraries and some of the archives in Prague but little from manuscript documents there in general and nothing from the Central State Archive. His book does make an important contribution to the intellectual history of the Czech-Jewish and Prague Zionist movements, but valuable work can still be done on the development of modern social patterns and national identification among the Jews of the Czech lands.

GARY B. COHEN
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MARTHA BOHACHEVSKY-CHOMIAK. *Feminists despite Themselves: Women in Ukrainian Community Life, 1884-1939*. Edmonton, Alb.: Canadian Institute of Ukrainian Studies; distributed by University of Toronto Press, Buffalo. 1988. Pp. xxv, 460.

"Feminist" was a word not often heard in Ukrainian political discourse in the nineteenth and early twentieth centuries, but Martha Bohachevsky-Chomiak believes that some Ukrainian women merited the title, despite themselves. In this wide-ranging and well-documented work, she looks at the public activities of women in Ukrainian lands from 1884 to 1939. The women she considers were in the Russian empire and the Soviet Union, in the Austro-Hungarian empire and later under Polish, Hungarian, or German rule, and even in the United States and Canada. She culls her data from a wide variety of publications and records found in archives scattered over the world from the Soviet Union to Canada. Her sources are, of course, written in a variety of languages. She also uses private letters, memoirs, and manuscripts and has conducted interviews with five prominent Ukrainian women leaders. A highlight of the volume is the collection of remarkable posters and photographs of individual women and of women's groups. There is little wonder that the book was ten years in the making.

In Bohachevsky-Chomiak's reconstruction, Ukrainian women were active in a wide variety of social projects, although none of them involved a recognizably feminine agenda. The work they undertook was not charitable and not directed to reform of the entire society, still less to women's liberation. Spontaneous in their appearance and, for most of the period, local in their activities, women's groups sought primarily to help Ukrainian communities both materially and culturally. Lacking political autonomy and even freedom in most regions, the Ukrainians faced the prospect that their culture would be suppressed by that of their colonial masters. Women accordingly organized rural cooperatives; supported schools for the teaching of cooking, sewing, and trades; and agitated for instruction in the Ukrainian language from grammar school to the university. The defense and nurturing of their historic culture commanded highest priority for Ukrainian men and women. The political environment was generally more favorable for women's activities in areas ruled by Austrians and Czechs than in those under Russian or Polish authority.

Ukrainian women had little to say about specifically feminine issues, and none of them elaborated a feminist theory. They wanted not equality but opportunity, the chance to advance the national welfare of the Ukrainian people and to preserve its culture. Always the liberation of the nation prevailed over the liberation of women, patriotism over awareness of gender.

Because they gave priority to nationalist over feminist goals, their activities bear little resemblance to the contemporary feminist movements in Western Europe and America. The Ukrainians rather resemble the

goals and activities of women in the present-day Third World, who similarly, the author maintains, place greater emphasis on development than on the achievement of feminist goals.

Although the women's movement in the Ukrainian communities for long remained locally based and lacked a common ideology—an international organization did not appear until the 1930s—women did maintain a high level of activity after the 1880s. This book recounts the accomplishments of numerous women. Among the most interesting is Natalia Kobrynska, a Galician socialist who advocated universal suffrage and higher education for women and, unlike her peers, founded day-care centers. Although a socialist, she never assumed that a socialist society would automatically endow women with equal rights. She called for battle not only against the oppressive social order but also against the age-old concepts that ascribed to women a meaner place in society than men enjoyed. She was very much ahead of her time, and only recently have researchers (including Bohachevsky-Chomiak) won for her the recognition she deserves.

Notable among the feminine activists was Milena Rudnytska, "without doubt one of the most prominent Ukrainian women of modern times" (p. 164). Born in the western Ukraine, she helped organize famine relief for the Soviet Ukraine in 1933 and was one of the founders in 1937 of the World Union of Ukrainian Women. An antifascist and fervent supporter of the League of Nations, she recurrently visited, in pursuit of her many interests, Vienna, Rome, Prague, London, and other capitals, where she met with world leaders.

The description of many local organizations involving many women is no easy task, and Bohachevsky-Chomiak accomplishes it perhaps as well as it could be done. Nonetheless, readers may regret the absence of maps on which the many place names she cites in the text could have been located. Occasionally, the number of women's names mentioned overwhelms the reader. Although she is understandably eager to include as many women as possible, it might have been preferable to give greater attention to fewer representatives of the movements she describes. Those to whom she gives space become memorable. The contrasts she discerns between the women's movement in the Ukraine and those in the West may be overdrawn. Women's movements in Western countries initially pursued active social programs before they developed feminist theories or agenda. Finally, the title is slightly disturbing. Some Ukrainian women, as Bohachevsky-Chomiak documents, resisted the title "feminist"; others embraced it. Neither group would likely have been pleased by the title's implication that they were the way they were "despite themselves."

This book remains a richly documented and highly informative work. Writings not only on Ukrainian but also on Eastern European women are few and not

often very informative. Bohachevsky-Chomiak deserves the special congratulations due to pioneers.

PATRICIA HERLIHY
Brown University

JOHN-PAUL HIMKA. *Galician Villagers and the Ukrainian National Movement in the Nineteenth Century*. New York: St. Martin's. 1988. Pp. xxxvi, 358. \$45.00.

John-Paul Himka scrutinizes how the peasants of Ukrainian Galicia developed and shaped their national consciousness. He identifies the modern western Ukrainian national movement with the peasants and argues that they became both the articulators and the beneficiaries of nationalism. This book provides a good illustration of how the social and national aspirations of the western Ukrainians coalesced. It should be useful in courses on the intellectual and social history of Europe.

The major difficulty in writing about peasants is the lack of sources that reflect their genuine voice. Himka overcomes that difficulty by using as his main source letters written or dictated by the peasants to the editor of the Galician journal *Batkovshchyna* (Fatherland) for the years 1884 and 1885 (it appeared twice a month at the time). He supplements the letters with documents that detail disputes between manors and peasants over rights to forests and pastures. He also uses background materials prepared for the Austrian administration on the treatment of the peasants by the landlords at the time of the abolition of serfdom in 1848–49 and the subsequent indemnity negotiations. In addition to the 222 pages of narrative text and a bibliographic essay, the book contains lists of peasant activists organized by categories.

Himka focuses on Ukrainian peasants; Poles and the thorny issue of Polish-Ukrainian relations appear in this work in so far as they impinge on the Ukrainian peasants. That is a useful approach to take, because the eastern half of Galicia was inhabited by the Ukrainians and the Polish presence there was limited to the upper classes, primarily landlords and government officials. Himka's graphic and detailed description of serfdom forms the background for understanding why Ukrainian peasants chose to become Ukrainian nationalists rather than revolutionary socialists.

The heroes of this book are peasant leaders—cantors, teachers, scribes—and the peasants themselves. Himka fully agrees with Austrian social democrat Otto Bauer, who wrote extensively on Eastern European peasants and who maintained that "it was the peasantry, a social class, which awoke and which carried its nationality forward with it" (p. 219). Himka frequently faults the Catholic priests and the teachers who were not of peasant origin, two groups generally considered the backbone of the modern western Ukrainian national movement, for opposing that movement. He also finds no evidence that priests played a significant role in helping peasants pursue legal cases on claims for

land and pasture privileges. Nevertheless, he maintains that, after 1848, the peasants, priests, and village notables formed an alliance for the pursuit of national and social goals. Through his close scrutiny of the court proceedings on the issue of the "servitudes," that is, the traditional rights of the peasants to take wood for fuel and construction from the forests and to graze their livestock in the pastures, Himka provides detailed documentation on the evolution of national consciousness among the peasants.

Along with their development of a national consciousness, the western Ukrainian peasants, through their dealings with the courts and the press, developed an appreciation of private property and of organized political activity. In the relatively brief period from the revolutions of 1848, which marked the abolition of serfdom, to the revolution of 1917, which witnessed the fall of the Austrian empire, western Ukrainian peasants became the backbone of a politically effective Ukrainian national movement. Himka attributes the failure of the Ukrainian state in the eastern Ukraine in 1918–22 to the failure of similar national development among the eastern Ukrainian peasants. The Russian government consciously impeded effective community organizations, as well as the spread of schooling, among the Ukrainian peasants. Hence, those peasants, although they formed the majority of the population in the Ukraine and although they spoke beautiful Ukrainian, lacked the national consciousness and the political experience to become a genuine and effective force in the territory.

This book provides another argument, if one is still needed, to put to rest the ridiculous assertion that the Ukrainian movement was an Austrian invention and the equally unsubstantiated charge that the western Ukrainian national movement was a product of the intelligentsia and did not have the interests of the peasants at heart.

MARTHA BOHACHEVSKY-CHOMIAK
McLean, Virginia

G. A. SANIN. *Otnosheniia Rossii i Ukrainy s Krymskim khanstvom v seredine XVII veka* [The Relations of Russia and the Ukraine with the Crimean Khanate in the Middle of the Seventeenth Century]. Moscow: Nauka. 1987. Pp. 270. 2 r. 30 k.

Even more than two decades after William H. McNeill called for serious historical attention to seventeenth-century Eastern Europe in his important book *Europe's Steppe Frontier* (1964), this period and region have remained largely untitled by scholars. A subject enormously complicated by major institutional and cultural changes sustained by all states in the area, the seventeenth century provides challenges that only the most skilled and sophisticated historian can hope to meet.

G. A. Sanin has attempted to take on these challenges with a new study (in the chronological but not methodological sense) of the relations between three of the

East European participants in this period. Making use of heretofore unavailable or underused Soviet archival materials, Sanin has also found Polish and Ukrainian sources that help elucidate complex issues. Unfortunately, nothing from Istanbul or from the khanate archives, lost since the revolution and not recovered (or made public), was consulted.

Sanin takes as his point of reference the view that Russian-Muscovite foreign policy was defined through the identification of "tasks" (not a new historiographical approach) and that effective analysis of this policy is best produced by judging tasks completed and tasks postponed. Further, Sanin asserts that Russian and Ukrainian achievements and Crimean Tatar and Ottoman failures were all positive developments for the future of East Europe. An inevitable outcome of this approach is the further assertion that Tatar and Ottoman developments are historically significant only to the extent that they influence Russia and Ukraine. That the Tatars maintained a state and society of their own, with their own "tasks," Sanin not only ignores but denies.

The major Russo-Ukrainian tasks of the century, according to Sanin and most Russian historians before him, were the unification of Ukraine and Belorussia with Russia; open access to the Baltic; and, closely related to the first, extension of the Russian frontier southward and defense of it from Crimean Tatar raids. The first and third were largely accomplished in the seventeenth century, to "everyone's benefit" (p. 241), while the second task was postponed until the beginning of the next century.

Despite his access to new archival sources, Sanin has produced no startling evidence; his conclusions remain traditional. As with so much else in modern historiography, Sanin's book would have benefited if he had consulted some works written by his colleagues, particularly those in France (Alexandre Bennigsen, Gilles Veinstein, and others), Turkey, the United States (Halil Inalcik), and Great Britain (Metin Kunt). There is no evidence from his bibliography or text that Sanin is part of a truly international historical profession.

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STEPHAN M. HORAK. *The First Treaty of World War I: Ukraine's Treaty with the Central Powers of February 9, 1918*. (East European Monographs, number 236.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1988. Pp. vii, 202. \$25.00.

Numerous textbooks mislead readers into thinking that there was only one treaty signed at Brest-Litovsk in 1918—that concluded by V. I. Lenin's Soviet Russian Republic on March 3. An earlier treaty, however, signed by representatives of the Ukrainian People's Republic on February 9, 1918, was the first peace treaty of World War I and had important consequences.

Unable to obtain aid from the Entente Powers and confronting an invasion by Soviet Russia, the Ukrainian Republic's most viable option was to conclude a peace treaty with the Central Powers. The treaty discredited Leon Trotsky's policy of "neither war nor peace." It also forced Lenin's hand, as he had commenced peace negotiations but was now confronted with a Ukrainian delegation that he could not control. The treaty not only granted Ukraine international recognition but also served as a precedent for the "containment" of Bolshevism and contributed to limiting Soviet Russia to the pre-Petrine European borders. Lenin was also compelled to deal with the new Ukrainian state. The Ukrainian diplomats, who were probably the most youthful negotiators of a peace treaty in modern history, acquitted themselves quite well and were able to obtain concessions from the Central Powers.

This posthumously published work is Stephan Horak's substantially revised and expanded doctoral dissertation, which was defended at the University of Erlangen in 1949. In revising the work, Horak made excellent use of archival data in Vienna, Koblenz, and Bonn that provided revealing findings regarding German and Austrian motives and disagreements and their perceptions of the Ukrainian Central Rada. The work includes, for example, the substance of reports prepared by Austrian military intelligence and by Ambassador Johann Forgach concerning conditions under occupation by the Central Powers (pp. 116–18, 121–23).

Apart from the original archival data, Horak has placed the treaty in its broader context and has addressed important questions. He is critical of the socialists in the Ukrainian Central Rada who did not understand the importance of an adequate national defense and a trained military force and who "disarmed their own socialist revolution" (p. 146). His archival sources reveal the essentially one-sided and exploitative policies of the German military and economic officials. Horak criticizes the Central Powers for their short-sightedness in not organizing a Ukrainian army and for thinking in terms of short-range economic benefits rather than in terms of long-range political objectives. He gives ample attention to the Ukrainian State headed by Hetman Pavlo Skoropadsky. He is also critical of the Entente Powers for their failure to foresee the long-range consequences of the nullification of the Brest-Litovsk treaties, the significance of the military build-up undertaken by Lenin while the treaties were in force, and the implications of the rapid withdrawal of the German military in late 1918.

The volume includes a valuable historiographical chapter that surveys and evaluates the German, Austrian, Ukrainian (Soviet and non-Soviet), and American literature dealing with the treaty. Appendixes include the Ukrainian declaration of independence—the Fourth Universal issued by the Central Rada on January 22, 1918, the text of the February 9, 1918 Treaty of Brest-Litovsk, and the secret agreement

between Austria-Hungary and Ukraine (which Vienna did not honor) concerning the establishment of a Ukrainian Crownland (East Galicia and Bukovina) within the Dual Empire.

Horak's work provides an important and necessary corrective to John W. Wheeler-Bennett's *Forgotten Peace* (1939). It contributes significantly to our understanding of Ukrainian developments in the twentieth century. It should be read and heeded by errant textbook writers.

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HUGH RAGSDALE. *Tsar Paul and the Question of Madness: An Essay in History and Psychology*. (Contributions to the Study of World History, number 13.) New York: Greenwood. 1988. Pp. xviii, 266. \$39.95.

The following facts are not in dispute: Emperor (and not tsar as in the title) Paul I was unpredictable, moody, capricious, suspicious, despotic, and maniacally petty. He acceded to the throne at the age of forty-two, having been kept away from any governmental role by his mother, Catherine II, who was jealous of her power. He was intent on reforming the administrative and economic conditions of the empire in order to bring happiness and prosperity to all. In spite of these good intentions, his personality and political clumsiness often proved counterproductive. Eventually, a conspiracy of highly placed courtiers and officials assassinated him. The conspirators justified their bloody deed by claiming that the emperor was mad.

Although no historian, I think, has ever taken this self-serving explanation seriously, Hugh Ragsdale has written this study to examine whether indeed Paul I was mad. The author also makes the curious argument that, even if Paul had been mad, it was not the practice in eighteenth-century Europe to kill insane monarchs, so that there was no justification for Paul's assassination. This exercise might have been worth undertaking if Ragsdale had provided a convincing (or novel) explanation for Paul's behavior and for his overthrow and murder. But, as a matter of fact, the author is not really concerned with this task, for he contributes neither facts for greater knowledge nor interpretations for a better understanding of the many unclarified and debatable aspects of Paul's reign (administrative reforms and practices, policy toward serfdom and the peasantry, diplomacy and military involvements abroad). *Lieutenant Kizhe* remains still the best and most readable description of Paul and his reign.

Ragsdale's methodology is somewhat peculiar; it is as if in order to repaint a room he has matched existing colors against a color chart to find the best fit. Thus, he goes through a lengthy list of eighteenth-century and contemporary medical labels attached to a number of psychological phenomena to see which ones best fit Paul I. He concludes that, in modern psychiatric terminology, Paul's behavior would be classified as obses-

sive compulsive, with a degree of paranoia, but only at a neurotic, not a psychotic, level of intensity. Similarly, to characterize Paul politically and intellectually, he holds the emperor's dicta and acta against a list of concepts that Ragsdale believes to have been prevalent at the time, and he concludes that Paul was predisposed to the *Polizeistaat* rather than to the ambiguous message of the Enlightenment. Attaching labels and inferring "influences" from books owned or available, in good Lansonian fashion, does not account for the context or provide a "thick description" that may yield meaningful explanations. Ragsdale's mechanical and superficial methodology may perhaps be accounted for by his insufficient familiarity with the cultures of eighteenth-century Europe and his quite impressive knowledge of contemporary psychological and psychiatric literature. There are a number of mistranslations and confusing quotations. A howler is the author's attribution to Maria Fedorovna (Paul's second wife) of the authorship of verses describing her ideal of a woman's role that are, in fact, taken from Chrysale's famous monologue in Molière's *Les femmes savantes* (II, 7).

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EMERITUS
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RONALD GRIGOR SUNY. *The Making of the Georgian Nation*. (Studies of Nationalities in the USSR.) Bloomington: Indiana University Press or Hoover Institution Press, Stanford. 1988. Pp. xviii, 395. Cloth \$45.00, paper \$17.50.

The overriding theme of this admirable synthesis is that, throughout its long history, Georgia has rarely been able to determine its own destiny. Ronald Grigor Suny shows how, during its period of relative independence until the beginning of the nineteenth century, Georgia was confronted almost continually by strong and aggressive neighbors: Rome, Byzantium, Iran, the Arab Caliphate, the Seljuk Turks, the Mongols, the Ottomans, and the Safavids. He measures the influence of each group on the political, cultural, and economic development of Georgian society. But he gives equal attention to those internal elements that formed its backbone. Of primary significance was Christianity, which fused with local customs and traditions to create a cultural and spiritual force that differentiated its believers from both Muslims and Armenian Christians. The overwhelmingly agrarian character of Georgian society also helped to ensure ethnic and institutional continuity over the long haul. Suny's description of rural society, especially of the Georgian variety of feudalism, illuminates the strengths and weaknesses of autochthonous economic and political institutions.

Suny devotes the bulk of his work to Georgia's connection with Russia, tsarist and Soviet, after its incorporation into the Russian empire in 1801. Of particular interest is the account of the steady transformation of Georgian society during the nineteenth

century caused by industrialization, a growing market economy, and administrative reforms. Agrarian relations receive special attention. Suny shows how Georgian serfdom, which had traditionally been an informal arrangement between serfs and landlords, assumed a new rigidity under Russian administration. He then describes the emancipation of the serfs in Georgia, which he calls a disaster because it left the peasants thoroughly dependent on the nobility.

Russian authorities contributed unwittingly to the development of national consciousness. Suny describes how their policies enhanced a sense of national identity by uniting the various regions of Georgia economically and politically and by forcing the Georgians to define themselves ethnically, because of their increasing contacts with foreigners, especially in the growing urban centers. The account of the Georgian national movement—the role of intellectuals, their concern for language and history, and the emergence of political ideologies that disrupted their initial unity—is reminiscent of similar movements in Eastern Europe. Perhaps Suny could have paid more attention to Western influences on Georgian intellectuals, particularly "Europeanism" in literature and social thought. Of great value is the discussion of how Marxism gained popularity among diverse classes by offering solutions to Georgia's major problems, which eluded populism, liberalism, and traditional nationalism. By 1905 the Marxists had become national leaders because they provided urban workers, peasants, and nationalists with a strategy for combating the common enemy, Russian bureaucrats and the Armenian middle class. The ethnic character and mass peasant support of Georgian social democracy made it unique among socialist movements in the Russian empire.

In his chapters on the Soviet period, Suny provides the best account of the subject in English. The impact of Stalinism on the development of Georgia is evident everywhere. As Suny demonstrates, the Communist transformation began with the application of a new nationality policy in the 1920s and continued with industrialization and the brutal destruction of the kulaks and the collectivization of agriculture, which destroyed the peasantry as an independent political and social force. Those economic campaigns were accompanied by an equally momentous cultural revolution that aimed at creating a new, loyal intelligentsia from the working class. Curiously, increased pressure from Moscow, which was accompanied by the promotion of the Russian language and the increased employment of Russians at the all-union level, left the titular nationalities in the republics in a dominant position. As a result of this "re-nationalization" (p. 298), Georgians reaped benefits in every area of public life, a trend that Suny foresees continuing beyond the 1980s.

Suny keeps the reader's attention focused on the principal threads of Georgian history and provides ample detail to follow its evolution. Perhaps he could have given more space to economic and social institutions and to intellectual and literary life in the Stalinist

and post-Stalin periods. Nonetheless, we have here the standard account of Georgian history in English.

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JEAN-FRANÇOIS BOURRET. *Les Allemands de la Volga: Histoire culturelle d'une minorité, 1763-1941*. Lyon: Presses Universitaires de Lyon and Centre Nationale de Recherche Scientifique. 1986. Pp. 529. 180 fr.

Jean-François Bourret's book is a study of the Volga Germans that spans a period of almost two centuries. The work is organized in two parts. Part 1, composed of four chapters, covers the period from 1763 to 1917; part 2, composed of three chapters and a conclusion, is devoted to the years between 1917 and 1941 of the Soviet period.

The main purpose of chapter 1 (part 1) is to review the historical background of the German colonization of the Volga region. Bourret identifies the most important issues affecting the outcome of colonization from the provisions of the 1763 Manifesto of Catherine II to the methods of immigrant recruitment employed by the Russian government, as well as the reaction of the German states to immigration, the background of the immigrants, their demographic evolution, and internal administration in Russia. Bourret's discussion of nineteenth-century legislation affecting the status of the German settlers, in general, and the discussion of the 1871 law, in particular, are descriptive rather than analytical. The integrative impact of the 1871 law is stated rather than thoroughly analyzed, not even in the comparative context of similar legislation affecting other minorities. A critical approach to the manner in which the fragile balance between new rights and new obligations stemming from the legal developments of the 1860s and 1870s affected the identity of the Volga Germans would have greatly contributed to a better understanding of the emergence of a Volga German identity. An inquiry into the impact of the unification of Germany on the reassessment of the identity of the Volga Germans vis-à-vis Russia, as well as vis-à-vis the land of their ancestors, would have further enriched Bourret's discussion of the complex issue of national identity.

Although Bourret provides a detailed description of Lutheran and Catholic parishes, legislation affecting churches, confessional and private schools, school reforms, and the emergence of the German press in Russia, in chapters 2, 3, and 4, he falls short of addressing the issue of the dynamic relationship between the religious and the secular in the emergence of a modern national identity among the Volga Germans.

Part 2 follows the descriptive model of part 1, and neither the background chapter nor the chapters on education and communication media identify clearly the components of a Volga German identity, the man-

ner in which it was articulated, and the degrees of assertiveness that characterized it.

Most cultural historians reach out for information provided by belles-lettres, folklore, ethnography (to name a few areas), in addition to relying on information concerning religious life, education, and the press, in search of answers to questions regarding the cultural anatomy of an ethnic, socioeconomic, political entity. Bourret does not.

Lacking a bibliography and an index, this volume features, on pages 513-17, maps illustrating the administrative organization of the Volga Germans and the network of Lutheran and Catholic villages and parishes. Preceding the maps is a puzzling section entitled "Biography." This is neither an exhaustive name index nor an appendix providing biographies of leading Volga Germans. It contains entries such as Bela Kun and N. Krupskaja along with A. Rothermel and T. Weber, entries excelling in a brevity bordering on laconism (K. Radek), as well as page-long presentations (A. Schneider).

The above observations notwithstanding, Bourret's book is a welcome addition to the growing number of studies dedicated to the ethnic mosaic of the Russian and Soviet states and a useful source of information for students of ethnicity and nationalism.

AZADE-AYSE RORLICH
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O. NIEDRE. *Sotsial'naiia struktura krest'ianstva Pribaltiki (V pervye desiatiletiiia XX veka)* [The Social Structure of the Peasantry of the Baltic: The First Decades of the Twentieth Century]. Riga: Avots. 1986. Pp. 175. 1 r. 20 k.

Rarely has a work of social history been so contorted by official ideology as this study of the Baltic peasantry. That such a social history would be published under the auspices of the Institute of Party History of the Communist Party of Latvia, itself a branch of the Institute of Marxism-Leninism of the Central Committee of the Communist Party of the Soviet Union, is enough to raise suspicions. These suspicions are immediately confirmed on the first page of text when the reader is informed of the need "to expose bourgeois falsifiers of history" and "to overcome mistaken notions about the patriarchal nature of peasant life" (p. 5). This statement, O. Niedre unabashedly informs us, is in accordance with the views of the Plenary Session of the Central Committee of the Communist Party of the Soviet Union of July 14, 1983. It certainly does not derive from any reading of the sources.

Initially, this work seems bad, old history Soviet style. Niedre's conclusion is straightforward: "All the factual material confirms that the process of social differentiation of the rural population in the Baltic corresponds to Lenin's theoretical concepts on the capitalist development of agriculture" (p. 131). Thus, he attests, as have many before him, to the applicability of these

ideas to regions with quite diverse social structures, demographic patterns, ecological determinants, economic organization, and political traditions. Niedre's entire text purports to show that, in spite of what seem unique aspects of Baltic peasant life, they are not of consequence in the larger context.

Niedre attempts to illustrate that an emerging peasant bourgeoisie, and not a noble landowning elite, was responsible for progressive forms of agrarian development. Significantly, he argues that it is the level of marketed product that best identifies the well-to-do peasant household. This is a clear rejection of such measures as area sown, livestock holdings, and use of hired labor, widely employed by Soviet historians and derived from Lenin's *Development of Capitalism in Russia*. Yet, if this is a critique of Lenin's method, the discussion follows a tortured path with language so Aesopian that no one can be sure what is implied. In any case Niedre, by never employing regression analysis, is unable to prove his point. Thus, his concept of marketability as the key component of the kulak household economy, although of some potential, remains speculative.

It is in Niedre's description of the diversity of Baltic agrarian structures that this work has merit. The Baltic region was distinguished by its huge mass of landless peasants. But such peasants, Niedre rightly emphasizes, were far from homogeneous and included wage laborers on noble estates, wage earners on peasant lands, cash renters, sharecroppers, renters who compensated landowners with labor services, and an unsettled population caught between town and country. Thus, Niedre implies the limitations of the simple concept of rural proletarianization widely used by Soviet historians.

In all, this is a confusing work. At times Niedre offers a telling critique of traditional Soviet approaches to studying peasant society. Yet his own measures are speculative and his conclusions in accordance with established views. Perhaps there is more here than meets the eye. Perhaps there is not.

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ABRAHAM ASCHER. *The Revolution of 1905: Russia in Disarray*. Stanford: Stanford University Press. 1988. Pp. xii, 412. \$39.50.

In the annals of revolution, the upheavals of 1905 are universally classified as a failure because the autocratic system survived the challenge to its legitimacy and persisted in ruling Russia for another dozen years. The most dramatic episodes took place during 1905, but the revolution required three years to run its course, beginning with the reform campaign mounted by liberals in the fall of 1904 and concluding with the Stolypin coup d'état in June 1907. Given the tremendous significance of those events for Russia's history and, more generally, for the course of social change in

the twentieth century, it is astonishing that no comprehensive account of the 1905 revolution has hitherto been available to Western readers. The sole exception is a short history by Sidney Harcave, originally published under the title *First Blood* and later retitled *The Russian Revolution of 1905*, which first appeared in 1964.

Abraham Ascher's book finally gives the events of 1905 their due in Western scholarship. The first volume of a two-volume work, it covers the period from late 1904 to the end of 1905. This is a book to be savored. It is broadly conceived to encompass the revolution "from above" as well as "from below," analytically strong with a powerful and persuasive argument, and gracefully written with a compelling narrative. It incorporates a vast array of primary sources and secondary works, many of them previously untapped. The study has much to offer specialists but would also be a splendid addition to certain courses (the book deserves a paperback edition) and will appeal to a broad audience interested in problems of revolution and social change.

Ascher adopts a distinctive approach to the causes, processes, and consequences of the revolution. Eschewing interpretations that focus on impersonal forces or structures, he emphasizes the decisive role of human agency. This is a book about how people shaped history by virtue of their ideas, beliefs, and allegiances. To be sure, they were affected by overarching economic, social, and cultural changes brought about, in large measure, by the government's program of modernization. But those changes were significant, in Ascher's view, precisely because they influenced the way people apprehended the world. Time and again, Ascher directs the reader's attention to the choices that confronted individual actors and groups—decisions that were made for complex and sometimes inexplicable reasons but that had far-reaching consequences.

This book places the liberal intelligentsia, rather than workers or peasants, at the center stage of the upheavals. Ascher commences his account with a discussion of the key role played by liberals in creating a new political climate in late 1904. In the months following Bloody Sunday (January 9, 1905), liberals sought government reform, but the authorities repeatedly offered too little too late. Profoundly frustrated by the government's inflexibility, liberals lent their support to the general strike in October, thereby compelling the tsar to make major concessions. The October Manifesto granted many of the reforms that liberals had been demanding, but by then liberals were unwilling to abandon their "militant hostility toward the government" (p. 248). Actions by Prime Minister Sergei Witte further alienated reform-minded elements and rendered cooperation impossible. Many liberals also drew apart from the lower-class movement in the cities as the newly established Soviets of Workers' Deputies generated increasingly radical demands. "The deepening conflict between the government and the revolutionary left placed the liberals in a precarious position" (p. 293);

their appeals to the government for reform and to the militant leftists for moderation fell on deaf ears. The final turning point in 1905 was the Moscow uprising: "The impact of the Moscow uprising on the course of the revolution can hardly be exaggerated. As a result of it, the liberal movement, which until October had played a decisive role in undermining the autocracy, became politically more cautious and increasingly distanced itself from the radical left" (p. 325). Savage reprisals by the government in the wake of the uprising and continuing into the winter and spring of 1906 ended "the chances for a reconciliation between society and the government" (p. 335).

The central argument of the book, which I find generally convincing, is that the revolution is a tale of missed opportunities by the government to accept and implement liberal reforms in a timely fashion. The outlook of the tsar and his advisers blocked the possibility for reform: they lacked "vision and boldness" (p. 207), and they suffered from "intransigence and narrow-mindedness" (p. 342). Although they are the villains of the story, they are nonetheless portrayed as victims of their own limited perspective and their unwillingness to relinquish prerogatives. If there are heroes here, they are the liberals who attempted in vain to steer Russia toward constitutional monarchy. Their efforts were undermined by the government on the one hand and by radical parties and lower-class groups on the other.

Lest the foregoing outline of the main argument unduly stress Ascher's account of politics "from above," it must be emphasized that this study also deals extensively with politics "from below," that is, with the aspirations and activities of lower-class groups (workers, peasants, soldiers) and with the impact of the revolution on the lives of ordinary people (for example, the book includes illuminating discussions of such topics as the local police and pogroms). Here, too, the author draws our attention to changes in mass consciousness (especially following Bloody Sunday and the October Manifesto) and to the critical role of ideas propagated by radical activists. In these portions of the book, as elsewhere, Ascher gives us a masterful study of a revolution that appeared to many contemporaries to have reached a tragic conclusion by June 1907. That is a perspective Ascher clearly shares. For the second part of the story, we must await the next volume of this monumental work.

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ROBERT W. THURSTON. *Liberal City, Conservative State: Moscow and Russia's Urban Crisis, 1906-1914*. New York: Oxford University Press. 1987. Pp. viii, 266. \$32.00.

Robert W. Thurston's work is a social and political history of the Moscow city administration in the years

before the Great War. He emphasizes the struggle between reform-minded urban elites and the central imperial government. Basically he argues that the Moscow urban elites took considerable care both before and after 1905 to solve the numerous social problems caused by rapid urbanization within an essentially agrarian framework. They were ultimately stymied, however, by the bureaucracy of the central state with its ideology of police hegemony, its social connections to the elites of rural Russia, and its commitments to the ideology of the old regime. In this sense, Thurston's argument dovetails with that of Roberta Manning's *The Crisis of the Old Order in Russia* (1982) and the works of other students of Leopold Haimson, which equate the old regime with the values of the provincial landed gentry and its symbiotic relationship to the central state. In Thurston's view, ideology was the "fundamental source of the dispute between the liberals and the state" (p. 184).

Autocratic ideology led the central government to opt for agrarian solutions, to promote the estate (*soslovie*) system, and to reject genuine decentralization and rule of law. Even the genuine attempts to improve municipal services, the funding, and the infrastructure of cities in the policies of such reformers as Sergei Witte and Petr Stolypin amounted to little because the deepening pre-World War I problems (demographic, economic, and the like) overwhelmed the central state's resources and commitment to change. For Thurston, the result is a textbook study in the classic split between state and society, with the formerly loyal urban elites (largely the Moscow Progressives and some Octobrists) appearing as gloomy oppositionists along the road to the February revolution. Thurston rejects the Soviet notion that the urban liberals wavered and then supported the regime in order to avoid revolution. Rather, these city leaders, well versed in the social realities of industrializing urban Russia, became fierce opponents of the autocracy precisely because they saw the central government's ill-conceived urban policy as about to produce the revolution.

This basic framework is hardly original, and indeed the value of Thurston's effort is not to be found in new formulations of the "crisis of autocracy." The book is extremely well researched, however, and does provide excellent discussions of Moscow's urban landscape and social profile (1906-14) and the structure and competence of municipal government, education, social services, and municipal finances (especially the structural causes of persistent shortfalls). Here Thurston argues that it was the state's political choices (failure to give cities adequate taxing power and siphoning urban resources to its own coffers) and not Russia's chronic poverty that resulted in budget shortfalls. The chapter on the social composition and politics of the "city fathers" is particularly rewarding. Thurston goes beyond statistical and legal categories to the question of occupation, and he makes a compelling case for the diversity and fluidity of urban society. Although the Moscow Duma deputies were largely businessmen and

professionals, Thurston maintains that their politics (either moderate right or Progressive) cannot be linked directly to either social origin or occupation. Again he makes the case for the primacy of politics, a refreshing and integrative point of view in a book that pays so much attention to social history. The rich discussions of the municipal police and labor policy round out a very successful synthesis of social and political history in the urban setting.

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N. G. DUMOVA. *Kadetskaia partiia v period pervoi mirovoi voyny i Fevral'skoi revoliutsii* [The Kadet Party in the Period of the First World War and the February Revolution]. Moscow: Nauka. 1988. Pp. 244. 3 r.

N. G. Dumova's impressive work on the Russian Constitutional Democrats (Kadets) reflects much of what is occurring in contemporary Soviet historiography. This book was written a number of years ago and only released this year. It is characterized throughout by an exceptional familiarity with Western literature and is as much a learned review of American, English, and French historiography as it is a scholarly exploration of the Kadet party's outlook and high politics during World War I and the revolution. Dumova also engages A. Ia. Avrekh, E. N. Burdzhakov, and other Soviet authors by offering her own distinctive interpretations drawn carefully from the archives. She is the first historian to make extensive use of the Russian émigré archive from Prague, now an uncataloged "collection" in the Central State Archive, and her familiarity with published and unpublished materials contributes to an unusual understanding of Kadet leaders as real and complex people. Dumova's careful reference to the death of Pavel N. Miliukov's son on the Galician front during the first weeks of the war, for example, necessarily sets her analysis of Miliukov's role as foreign minister in a humanistic context.

We learn a great deal from this volume, which covers 1917 as well as the years of World War I, about the Kadets' broadening influence through personal contacts during the war with the so-called public organizations; about the activities of provincial committees before and after February; about important Central Committee meetings, especially in June and August; and particularly about the divisions and strains that continually rent the Kadet leadership. Using the extensive correspondence in the Prague collection as well as the manuscript memoirs of V. A. Maklakov, F. F. Kokoshkin, V. A. Obolenskii and others, the author also tells us much about individual Kadet views in 1917 on V. I. Lenin and the Bolsheviks, coalition government, General L. G. Kornilov, and the revolutionary situation in general. Dumova indicates, for example, that Miliukov and other Kadets thought very early in 1917 that Lenin was a far greater threat than many realized and that some, such as Ivan Petrunkevich, later felt Alex-

ander Kerensky erred by not arresting the Bolshevik leader in May. She also strengthens an argument made in an earlier essay that, by early August 1917, the threat of military dictatorship "did not frighten the majority of Kadet Central Committee members," as Obolenskii's memoirs put it, and that concern centered instead on the question of whether "overturning the regime" (a *gosudarstvennyi perevorot*) would be successful.

Given when it was written, however, Dumova's careful exposition is still understandably constrained by familiar analytic categories and by a need to set the cultures and mentalities of Russian liberalism into a fairly narrow political matrix. The complex and contradictory ways in which the Kadets and the liberal intelligentsia as a whole did and did not reflect "bourgeois" values and interests is not an open question here, although one senses that the author could write at length on the subject, nor is the nature of liberal society and its relation to Russian revolutionary change. Dumova knows the people she writes about so well and is so clearly cognizant of the complex humanistic dimensions of revolutionary politics that one wants very much to learn her views on such questions as the relationship between the Kadets' civil war mentalities in the summer of 1917 and the longer patterns of Russian cultural development or the very character of Russian liberalism in its broader dimensions. We can only look forward to new and impressive contributions from Dumova and others as the principles of *glasnost'* are more firmly established.

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A. G. KAVTARADZE. *Voennye spetsialisty na sluzhbe Respubliki Sovetov, 1917–1920 gg.* [Military Specialists in the Service of the Republic of Soviets, 1917–20]. Moscow: Nauka. 1988. Pp. 276. 1 r. 80 k.

The Russian Civil War was a powerful and formative event in Soviet history. Yet, together with World War II, the two heroic war eras remain among the least studied and understood periods in the seven decades of Soviet history. This is, at least on the surface, hard to understand given the manifest importance of these eras. The Russian Civil War, which saw an improbable Bolshevik victory against overwhelming odds—large-scale Western aid to the Whites, significant White armies with strong tsarist officer components, disintegration of the country, repeated Allied intervention, and no Bolshevik experience with warfare—left an indelible mark on the interwar era. Recent works, such as those of Lynne Viola on collectivization (and earlier work by Stephen Cohen on N. I. Bukharin), have stressed the strong formative role of the Civil War and War Communism on the Stalinist programs of the 1930s.

But, if the West has paid relatively little attention to the Civil War—a situation only slightly alleviated by a

conference on the topic in 1985—the same is hardly true in the Soviet Union. Although the Civil War has naturally been eclipsed over time by the Great Patriotic War (in Soviet parlance), it retains a certain interest among Soviet historians. Many questions, such as the true role of Leon Trotskii and Joseph Stalin in the Civil War or the actual state of the White armies, have not been seriously addressed, but Soviet work on the period is definitely of value.

This new volume by Aleksandr Kavtaradze on the role of the tsarist officers (*voenspetsy*, short for “military specialists”) in the Red Army should be of considerable interest to Western specialists on military history in particular or Soviet history in general. There is much that remains problematic in the volume. The writing style is pedestrian and labored, and the spirit that infuses the study is still more that of the Brezhnev era than the Gorbachev era. In particular, many questions, such as those alluded to above, remain unanswered. Trotskii, the war commissar who pioneered and fought for the mass inclusion of the *voenspetsy* in the Red Army, is dismissed with five citations (relatively brief) in the volume. Lenin is treated with the reverence reserved for a demigod.

But there is much that is valuable. The mere question of using tsarist officers, 88 percent of whom at the level of generals came from the hereditary nobles as late as 1912 (p. 21), has remained problematic. Kavtaradze challenges some common notions in this volume. First, he argues that the great bulk of the *voenspetsy* were not openly hostile to the regime. He quotes data showing that only a handful actually committed treason during the Civil War (five out of eighty-two *voenspetsy* army commanders, p. 208). Kavtaradze demonstrates that the majority of *voenspetsy* did not fight for the Whites. Roughly 30 percent fought for the Bolsheviks, 40 percent fought in the White armies, and the remaining 30 percent fled the cities, emigrated abroad, took nonmilitary jobs, or died (p. 177). No less than 20 percent of the old generals and 7 percent of the old staff officers served in the Red Army (p. 177). Overall, 33 percent of all tsarist staff officers served in the Red Army during the Civil War (p. 196).

Second, Kavtaradze shows that the overall role of the *voenspetsy* traditionally has been underestimated. *Voenspetsy* during the Civil War made up 85 percent of the front commanders, 82 percent of the army commanders, 70 percent of the division commanders, and over 50 percent of the division staff officers. They were almost the entire front and army staffs and provided both commanders-in-chief of the Red Army (I. Vatsetis and S. Kamenev, p. 210). This was especially important given the very weak role of Communists (who lacked military training or experiences), who were, in his words, often “lone individuals in front and army headquarters (p. 211).

Third, he shows the crucial role of these *voenspetsy* in World War II. The regime drew heavily on the temporary *voenspetsy* (men who were drawn into the tsarist army only for the war) during World War II. No less

than ten of them were in the highest ranks during the war, including I. Kh. Bagramian, A. N. Vasilevskii, L. A. Govorov, F. I. Tolbukhin, and I. V. Tiulenev. And B. M. Shaposhnikov and A. I. Antonov, both career tsarist officers, served in the top Red Army staff (p. 225).

Finally, Kavtaradze corrects some other traditional notions. He shows that there were roughly seventy-five thousand *voenspetsy*, not forty-eight thousand, in the army at the end of the Civil War. He documents that twelve thousand former White officers served in the Red Army and that some, like Govorov, actually advanced all the way to marshal (p. 174). Overall, Kavtaradze provides some valuable data and correction of generally accepted ideas. The book is definitely useful to the specialist in the field and deserves a broader market than usual for such works.

JONATHAN R. ADELMAN
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ANNE D. RASSWEILER. *The Generation of Power: The History of Dneprostroi*. New York: Oxford University Press. 1988. Pp. viii, 247. \$32.50.

The value of local history for charting the Industrial Revolution in England and its later nineteenth-century West European and North American analogues has long been apparent to readers of this journal. Students of Soviet industrialization have been somewhat slower to adopt a similar approach, partly because of the inordinate role of the central state and its policies and partly because of the difficulties of getting at reliable and sufficient sources. Anne D. Rassweiler's book is therefore to be welcomed as the first scholarly treatment of Soviet industrialization to focus on a single enterprise, in this case, Dneprostroi, the construction of an enormous hydroelectric dam across the Dnieper River in Soviet Ukraine.

Built between 1927 and 1932, Dneprostroi was the first giant construction project of the Soviet regime and, consequently, was invested with great symbolic value. As was the case with Magnitostroi, Kuznetsk-stroi, and other major undertakings of the period of the First Five-Year Plan, it embodied the typically Bolshevik values of harnessing and transforming nature and human nature alike. “We build the dam, and the dam builds us” was the prevailing ethos inculcated among the tens of thousands of workers, mostly raw recruits from the countryside, who worked on the site.

Using the local party and trade union press, a wide range of technical literature, published documentary sources, and interviews with veterans of the project, Rassweiler takes us from prerevolutionary schemes to Lenin's emphasis on the importance of electrification, the intense regional rivalry to obtain scarce investment capital, the delicate negotiations with Western concerns that resulted in the appointment of an American construction engineer, Hugh L. Cooper, as chief consultant, and the Sturm und Drang of obtaining the

requisite equipment, recruiting and training workers, and building an infrastructure from scratch. As Rassweiler makes abundantly clear, there was a shortage of everything except crises. Workers battled against the elements; trade union and party organs battled against turnover, absenteeism, and other forms of labor indiscipline; chief engineer A. V. Vinter battled against the Moscow-based board of directors over strategy and resources; and everyone battled against time. All of this is skillfully related to national developments—political struggles at the top, collectivization, shifts in wages, and incentive policies—that impinged on Dneprostroi.

There are, however, some important dimensions missing from this account. The author surprisingly has almost nothing to say about relations between foreign engineers and their Soviet counterparts or workers. She provides ample data on the fluctuating size and composition of the work force, amusing anecdotes extracted from interviews, and some fine illustrations, but Dneprostroi as a lived experience remains elusive. We are told that the population of Zaporozh'e, the nearest city, increased fivefold in a little more than a decade but are not told about life in the city, that peasants remained at the bottom of the status hierarchy within the work force but not how long or in what ways recruits remained "peasants" in others' or their own eyes. It is stated that party recruitment of "new, inexperienced, unsophisticated workers had an impact on the party" (p. 176) but not what that impact was.

Less excusable is the number of misleading statements about the historiography, the confusing references to institutions, and wrong dates. In the light of what has been published in the last ten years or so, studies by Solomon Schwarz, Theodor Von Laue, and Robert Conquest can hardly be considered "recent interpretations," and the claim that the impact of workers on labor policy "has been largely ignored or misunderstood" (p. 92) simply is no longer the case. Interchanging "ministries" and "commissariats" makes no sense, nor do references to a council of labor and security, a Komsomol-sponsored "Light Cavalier," and the engineering-technical personnel's "union." Finally, for the record, the Supreme Economic Council was abolished not in 1935 but in 1932, Stalin's renewed grain requisitioning in the Urals and Siberia does not date from the early spring of 1928 but from January of that year, and the Stakhanovite movement began not in 1934 but in 1935. The number of mistakes in bibliographic citations is also shockingly high.

In short, this is a pathbreaking book, but, like the massive undertaking it examines, it is bound to be improved upon.

LEWIS H. SIEGELBAUM
Michigan State University

LEWIS H. SIEGELBAUM. *Stakhanovism and the Politics of Productivity in the USSR, 1935–1941*. (Soviet and East European Studies.) New York: Cambridge University Press. 1988. Pp. xv, 326. \$34.50.

Once upon a time, the story of economic development in the Soviet Union in the 1930s was short, simple, and straightforward. Stalin masterminded the collectivization of the agricultural sector for the benefit of the growth of industry. There were "excesses" and "mistakes," but the general strategy was viewed as internally consistent and successful.

Now the story is much more confusing, thanks to scholars such as Lewis H. Siegelbaum. A phalanx of Western historians has been examining the 1930s in depth, and they have changed our simple story greatly. Siegelbaum's study of Stakhanovism is a significant contribution to the accumulating, excellent literature on this crucial period in the formation of the USSR. He offers us a detailed, chronological study of the development and ultimate institutionalization of Stakhanovism. His primary purpose is to help create a backdrop of social history to balance a story that has to date focused on the single personality of Joseph Stalin and the big events, such as the infamous great purges. Specifically, Siegelbaum's aim is to show that "Stakhanovism was both a state policy and a social phenomenon" (p. xii). He is successful in this effort, and anyone writing a history of the 1930s will perforce need to consult this very competent piece of scholarship. Given its frame of reference, the volume is comprehensive, thorough, and well organized.

Siegelbaum is interested mainly in the relationship between state and society, with explicit reference to Moshe Lewin's conceptualization (see pp. 9–10). I must confess that I found difficult to understand what the author describes as a "dialectical appreciation of the state-society relationship that informs the . . . study" (p. 10). And I am disappointed that Siegelbaum asserts that "the central issue . . . is not whether or to what extent Stakhanovism increased productivity, but how the issue of raising productivity was handled" (p. 11). Why not evaluate the economic consequences of Stakhanovism? What else but productivity was it supposed to accomplish? The author makes a good case for viewing the movement as "an important ingredient of the socialization of a largely peasant derived labor force" (p. 148). But this argument seems to evade Stakhanovism's principal purpose. In a system in which total individual wages depend on piece-work rates, a "curve buster" puts pressure on everyone to increase productivity.

The author does not make as much of his findings as he might. The Stakhanovite movement was played out as a typical Soviet-style campaign, as Siegelbaum's evidence makes clear. The creation of a Stakhanovite required state assistance, and the production records set were to a very large extent "rigged" by the enterprise and others. Materials were stockpiled. Equipment and backup support were concentrated. Every opportunity was seized to help Stakhanov break production records. And so it was also for his successors. The movement was glorified by the official press. It was characteristically, however, slowed by a "dizzy with

success" reaction. And, in the end, it became a patented Soviet fraud.

Siegelbaum is given to the use of rather abstruse Marxist terminology. Even Soviet scholars have begun to rely on general economic terms that are operationally defined. How can the author expect his general readers to understand what he means, for example, when he refers to "the constant capital content of production" (p. 302)? Even most economists will be mystified. He would be better advised to stick to well-defined terminology in the future. Fortunately, this weakness is only an irritant and does not undermine the strength of the book as essentially an empirical and atheoretical study of a major "reform" movement in Soviet social history.

JAMES R. MILLAR
George Washington University

NEAR EAST

S. D. GOITEIN. *A Mediterranean Society: The Jewish Communities of the Arab World as Portrayed in the Documents of the Cairo Geniza*. Volume 5, *The Individual: Portrait of a Mediterranean Personality of the High Middle Ages as Reflected in the Cairo Geniza*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xxx, 657. \$55.00.

Shelomo Dov Goitein died on February 6, 1985. He left behind an abundant legacy of scholarship transmitted through his students and publications. This volume, the completion of a project initiated in 1958, is a fitting consummation of a career spanning well over half a century. This tribute to a life of erudition is made all the more poignant because the final stages of publication were completed by Goitein's colleagues, students, and friends.

Earlier volumes have described the economy, the community, the family, and the daily lives of the Geniza Jews. In each of these areas, the Geniza materials were mined, and, from the massive data, Goitein extracted and refined a coherent portrait of the world in which the Jew of the medieval Arab East lived. In this last volume, we are offered a glimpse into the internal world of that Jew, a profile of the Geniza personality.

The volume is divided into four sections. The first explores the Geniza Jew as a social being. It relates the expression of self in social contexts and the needs for social satisfaction in coping with the challenges of life and death. Goitein notes, by way of example, that hospitality was a trait most conspicuous in the Geniza, but it was tied to a most sensitive, positive concern for the privacy of the guest. It is also interesting to note that in this society, in which up to one-quarter of its people might be classed as poor and which was also highly geared for provision of relief, "the people had no regard for spongers" (p. 78). People were expected to meet challenges with courage and self-respect.

The second section inquires into the characteristics of personality, desired and actual. It seeks to discover

the principal virtues and character traits that the Geniza Jew perceived as ideal. It treats the moods and sensitivities of the people and the need for recognition and appreciation that motivated their behavior. It also looks into the nature of friendship and enmity exhibited in the Geniza papers. This second part concludes with an interesting treatment of sexual attitudes as they existed among the majority of the population, which was Muslim, and of the ways in which those attitudes differed in the minority sector, which was Jewish. Not only does Goitein examine deviant sexual orientations, but he also seeks to understand the role of sexual abstinence in a community in which separation of spouses for extended periods was frequent. Goitein concludes, in this regard, that the prevalent attitude accepted the individual "as a harmonious aggregate of natural and supernatural inclinations" (p. 323).

The third part assays the Geniza person as a believer with a definite spiritual and moral character who displays trust and reliance on God. Further, the role of the community of the faithful in the support and growth of personality is treated. A discussion of the tensions attendant on the pluralistic realities generated by schism and counterreformation is also included. The section ends with an examination of the prevalent concepts relating to redemption, personal and communal, that is, messianic expectations and the world to come. Of some note here, in the context of the world of the true believer, is the manifest existence, in Geniza society, of strong feelings of powerlessness and insecurity. It was the Jew's confidence in the Merciful, however, that underpinned the ability to effect spiritual balance.

Finally, the last section provides a survey and seven specific portraits of the ideal of scholarship in a number of manifestations. This section culminates in an extensive sketch of Abraham Maimonides, who, for Goitein, "represented all the best found in medieval Judaism, as it developed within Islamic civilization" (p. 481). The volume then concludes with a valuable epilogue, tracing Goitein's journey from early training to the conception and creation of this work, and, as usual, copious and enlightening references and notes.

This series is already a classic, and this concluding volume is, I believe, the most exciting. It is less technical than the earlier volumes; the nature of the subject, personality, is not of the same texture as the earlier, more definitive areas. It is, however, perhaps even more speculative. Goitein exhibits ingenious skills of detection and interpretation (for example, p. 446), but, as he notes in one of his methodological comments, a definitive judgment requires definitive corroboration (p. 206). In that regard he challenges those who follow to explore the as yet underexploited resources of the Geniza and even suggests some likely and productive approaches. Aside from new insights offered in analyses of Sa'adya, Yehuda ha-Levi, and, of course, Abraham Maimonides, this volume provides an inspiring model of scholarly accomplishment and fulfillment. Goitein's personal and professional experiences are

shared in significant ways throughout the book. His concluding remarks about the Geniza are applicable to him as well. "Every happening of the past is a non-recurrent single episode; yet it has relevance to the general experience of mankind. . . . It is the privilege of the historian to conceive the accidental past as ever present life" (p. 502). This work will be read and reread, a monument of profound scholarship.

M. S. STERN
University of Manitoba

RESAT KASABA. *The Ottoman Empire and the World Economy: The Nineteenth Century*. (SUNY Series in Middle Eastern Studies.) Albany: State University of New York. 1988. Pp. xii, 191. Cloth \$39.50, paper \$12.95.

Resat Kasaba is a student of Immanuel Wallerstein's world-systems theory, which a recent critic (Leonard Binder) of development ideologies placed between structural Marxism (Louis Althusser, Nicos Poulantzas, and John Taylor) and the dependency and corporatist theorists (F. H. Cardoso, Enzo Falleto, and Guillermo O'Donnell) and the theories of revolution advocated by Barrington Moore, Theda Skocpol and François Furet. Wallerstein attempted to demonstrate that markets and politics do not necessarily coincide geographically, that international stratification is determinative of a country's or empire's economic development, and that countries or empires of the semiperiphery are important to the core and metropolitan (European) countries. Wallerstein posited further that states may move from the periphery and semiperiphery to the core and that core states may become peripheral. It is these possibilities that give optimism to scholars of the Third World for the economic development of their states.

Kasaba asks whether the Ottoman empire as a peripheral empire ever had the opportunity of becoming a core or at least a less peripheral state and one with a "civil society." He answers that the opportunity existed in the middle of the nineteenth century, but after the 1870s it faded because of the imposed bureaucratic centralism, especially during the reign of Abdülhamid II (1878–1909). This bureaucratic centralism was "institutionalized under the auspices of the core capital and core states of the capitalist world economy" (p. 115). This development led to the exclusion of indigenous, largely non-Muslim, capitalist intermediaries and the last best hope, according to Kasaba, of a "genuine civil society." Kasaba continues, "The relative isolation of the non-Muslims during the late nineteenth century was also significant politically in that it created the ground for the successful implementation of policies of natural enclosure [Young Turks] between 1908 and 1923" (p. 112). One has to assume that by logical extension Kasaba would also include the nationalist period of Kemal Atatürk. Such arguments are quite revisionist of the current historiography of this period. It seems unlikely that a civil society (Kasaba never

defines the term) or even an Islamic liberalism will materialize in the twentieth century in any country in the Middle East. Is it reasonable to postulate that such a civil society could possibly have emerged in the Ottoman empire in the middle of the nineteenth century and that non-Muslims would have been the fulcrum of that society? This is not to detract, however, from the prominence that Kasaba accords to the non-Muslims, especially in the Izmir area of western Turkey, in the development of capitalist enterprises in the late eighteenth and nineteenth centuries. He, like Çağlar Keyder, another scholar of Wallerstein's world-systems theory (*State and Class in Turkey* [1987]), rightly points to the significant role that non-Muslims played in the economy of the late empire.

It is perhaps somewhat ironic that the ability of the Ottoman empire to become more integrated into the European economy, even if it had no hope of becoming a core capitalist state, was prevented by the European Christian capitalist states that preferred the bureaucratic and Muslim centralization of Abdülhamid II, which allowed for the continued extraction of profits and revenues from the empire, to the competition of Ottoman, largely Christian, capitalists. It seems to me that Kasaba makes another contribution here (although he does not elaborate), that is, that the reign of Abdülhamid II, at least for the first decade of his rule, was supported by the European Christian capitalist states. Kasaba, more than any other scholar, provides the argument as to why this was the case, which should give pause to those historians who still think that Europe viewed Abdülhamid II as a despotic, tyrannical leader. If there was some truth to such views, perhaps it was necessary as part of the symbiotically capitalist relationship of the Ottoman empire with Europe.

Kasaba has, then, made a case for a revision of the historiography of the late eighteenth- and nineteenth-century Ottoman empire as accepted currently by many American, European, and Turkish scholars. It was the Christians of Europe and the Muslims of the Ottoman empire and not the Christians and Jews of the Ottoman empire that contributed the most to making the empire peripheral in the world capitalist economy, especially after the 1870s.

Many of Kasaba's arguments would have been stronger if he had been more familiar with the historiography of the period. For example, his periodization of the eighteenth century would have been greatly improved if he had used my study (*The Siege of Mosul and Ottoman-Persian Relations, 1718–1743* [1975]) and studies by Abdul Karim Rafeq and Karl Barbir, all of which address the same questions that he does but from different perspectives and with the use of different sources. It seems that world-systems theorists and historians of the Ottoman empire should read one another's works more closely.

ROBERT OLSON
University of Kentucky

ARTHUR GOLDSCHMIDT, JR. *Modern Egypt: The Formation of a Nation-State*. Boulder, Colo.: Westview or Hutchinson, London. 1988. Pp. x, 211. Cloth \$32.00, paper \$14.95.

This book is not a monographic study. Rather, it is intended as an introductory survey of modern Egypt for nonspecialists. Arthur Goldschmidt does a remarkably good job of compressing into a narrative of fewer than two hundred pages the essentials of the political history of modern Egypt.

The work's structure is chronological. After an introductory chapter briefly surveying Egyptian history prior to 1800, the first half of the book deals with the period from 1798 to 1952. The second half of the work is devoted to the post-1952 period, particularly the regimes of Gamal Abd al-Nasir and Anwar al-Sadat (the government of Hosni Mubarak receives only a few pages in the concluding chapter). The appended glossary, biographical dictionary, and bibliographic essay of works in Western languages should prove most valuable for the general reader.

The book is a very efficient introductory survey. The main developments in modern Egyptian political history are stated clearly, concisely, and by and large without obfuscating detail. Especially on the pre-1952 period on which Goldschmidt's previous research has centered, the book subtly incorporates the conclusions of recent scholarship into the narrative. Occasional anecdotes, local proverbs, and the political humor for which Egyptians are justly famous sprinkle and enliven the text.

For an account of a subject where so much is open to dispute, the work is unusually balanced and fair. Differing perspectives on several points of scholarly controversy are stated, and Goldschmidt's technique of attempting to summarize how Egyptians viewed major developments in their past—the British occupation, Nasir's Arab socialism, Sadat's opening to the West—makes for a sympathetic account of Egyptian history. Some of the specifics of Egyptian politics after 1952, where the historical evidence is still incomplete, are left fuzzy (Nasir's victory over Muhammad Nagib and others in 1954; the basis of Sadat's power in the 1970s in view of his movement away from Nasirism) or are open to different interpretations (the genesis of the Gaza raid of February 1955, where Egyptian encouragement of infiltration into Israel may not have been a factor; the motives for the 1973 war, where the stagnation of international efforts at mediation and the solidifying status quo in the occupied territories seem to be underestimated). But, on the whole, there is little to take issue with in the work's account of two centuries of Egyptian political history.

As emphasized above, the book is overwhelmingly a political history of modern Egypt. Although concise and generally accurate summaries of socioeconomic and cultural policies from Muhammad Ali to the present are provided, they compose but a small portion of the text. There is virtually no statistical data on the

Egyptian society and economy—probably a blessing for the general reader—but through such omission, at least in the case of the Arab socialism of the 1960s, the work may give the impression that the policies pursued brought more change than they did in reality. Readers who wish an analysis of the ramifications of Egypt's incorporation into the world economy in the nineteenth century, its increasingly complex class structure, or the ambiguous results of Nasir's and Sadat's socioeconomic policies will need to look elsewhere.

JAMES JANKOWSKI
University of Colorado

CHARLES D. SMITH. *Palestine and the Arab-Israeli Conflict*. New York: St. Martin's. 1988. Pp. viii, 308. \$35.00.

My survey of journal articles written in English about the modern Middle East indicates that more than one-third of the scholarly output since 1962 has focused on some aspect of the Arab-Israeli conflict ("The Study of Middle Eastern History in the United States," *The Jerusalem Quarterly*, 46 [1988]: 49–64). Most of those articles and monographs on the conflict have appeared since the Middle East war of June 1967. The war itself stimulated a resurgence of scholarly activity on the origins and evolution of the conflict. More important, renewed scholarly research about the conflict was undertaken after 1967 because archival documents that had previously been closed were finally available. In addition, time-use restrictions on primary source material had lapsed. The intersection of these circumstances prompted an avalanche of studies in the 1970s and 1980s on Zionism, the Palestine Mandate, the Palestinians, the Palestine Liberation Organization (PLO), modern Israel, and the personalities associated with the conflict's development and management.

Noticeably absent from the general literature on the conflict have been reasonably objective surveys incorporating the best of the most recent scholarship. The writing of this work by Charles D. Smith almost had to wait for the earlier studies to be completed. Compressing his book into fewer than three hundred pages, Smith succeeds in writing a comprehensive and relatively unbiased account. There are only occasional injections of personal opinion and some minor slights in emphasis. But the degree of subjectivity is benign in comparison to the general excellence of Smith's crisply written, descriptive narrative.

In correctly assessing the conflict's evolution, Smith devotes more than half of his composition to the period before Israel's establishment in 1948. He includes a brief but excellent summary of the Holy Land and the area that later encompassed Palestine in the ancient, Islamic, medieval, and Ottoman periods. Smith's review of the period from World War I to 1948 outlines not only the relentless Zionist dynamic and committed offensive to create a national home but also the tendency for the Arab population in Palestine to be repeatedly on the defensive organizationally and polit-

ically. In dealing with the pre-1977 period, Smith's objective restraint is admirably demonstrated, but, in the period when Menachem Begin and Ronald Reagan became central in Arab-Israel diplomacy, the author indicates an obvious disdain for the policies of both leaders.

Since the book's title emphasizes Palestine in the Arab-Israel conflict, a greater elaboration of issues consequential to the Arabs of Palestine would have amplified Smith's important work. He insufficiently covers the domineering role played by individual Arab states over the Palestinian question from the mid-1930s through the 1980s. A crucial theme in the unfolding Arab-Israel conflict is the Palestinian Arabs' struggle for personal identity and national autonomy within an Arab world that generally has treated them as refugees and as political pawns for parochial purposes. The book's contents tend to favor the primary roles played by the great powers and Israel in the conflict's evolution, and the work does not cover the official PLO attitudes on the issue of Israel's right to exist as a sovereign nation, especially in the context of the PLO's willingness to establish a "Palestinian national authority in any lands taken from Israel" (p. 235).

These points notwithstanding, Smith's book is a significant contribution to the historiography of the Arab-Israel conflict. As the conflict continues to unfold, Smith should be encouraged to update and revise this much-needed college textbook.

KENNETH W. STEIN
Emory University

AFRICA

ANN THOMSON. *Barbary and Enlightenment: European Attitudes towards the Maghreb in the Eighteenth Century*. (Brill's Studies in Intellectual History, number 2.) New York: E. J. Brill. 1987. Pp. viii, 173. \$44.50.

History, geography, and economics combined to make eighteenth-century Europeans more conscious of North Africa than of any other region outside their own continent. But centuries of interaction with the Maghrib had created negative European preconceptions, as evidenced by the terms applied to the area (Barbary) and to its inhabitants (Turks, a word known to be inaccurate but widely used because of its connotations of savagery, cruelty, and brutality). During the Enlightenment, however, the expectation that an understanding of non-European cultures might provide important insights to human social and political development induced intellectuals to go beyond those prejudices.

Numerous primary sources equipped them with the data needed to do so. The bibliography of this study lists more than two dozen European and American books describing North Africa in the century preceding the French conquest of Algeria in 1830. Those firsthand observers joined other Enlightenment thinkers in attempting to fit what was known about the

Maghrib into the systems being developed to classify newly acquired information on the world and its people. In analyzing Enlightenment perceptions of the Maghrib, Ann Thomson demonstrates that the region did not neatly fit into existing classificatory schemes. It was, for example, neither the seat of an ancient and respected civilization, as were India and China, nor a land of "primitive savages," as was the New World. Despite its Roman heritage, the Maghrib was not a part of the European (Christian) world, nor was it truly African. Its social and political structures were not in the state of nature that might illuminate the early stages of human evolution, nor were they sufficiently refined to offer any guidance to the more sophisticated Europeans. Political authority in the Maghrib seemed most akin to "Oriental despotism," but, in other crucial ways, North Africa struck European observers as quite different from the "Orient."

The Maghrib was such an anomaly that contemporary anthropological discourse frequently chose simply to ignore it. Even so, the more humane atmosphere of the Enlightenment generated, at least temporarily, a decidedly less prejudicial view of the region than had previously existed. Ironically, however, eighteenth-century Europeans' elevated consciousness of the remainder of the world led, by the century's end, to a sense of European superiority. Convictions based on the uniformity of human nature gave way to others based on the congenital inferiority of certain peoples, paving the way for the colonialism of the nineteenth century.

Because the Maghrib had never been definitively categorized by Enlightenment thinkers, it was relatively easy, given this shift in attitudes, to cast the region and its people in a negative light. As a result, adamant calls to suppress "Barbary piracy" and to "civilize" the southern shores of the Mediterranean replaced the more inquisitive and sensitive tone of earlier Enlightenment works in the several decades preceding 1830.

Thomson asserts that the adoption of this perspective required informed Europeans either to ignore or to reinterpret radically the body of information about the Maghrib accumulated in the previous century. Her work makes it clear that European readers did have access to the facts of North Africa's social, political, and economic conditions at the time of the French invasion of Algeria, but the picture of the Maghrib established in the Enlightenment was not one that justified European intrusion. As a result, Enlightenment views were cast aside in favor of a return to older stereotypes that not only left Europeans comfortable with North Africa's subjugation but also encouraged them to regard that process as a positive virtue.

KENNETH J. PERKINS
University of South Carolina

RALPH A. AUSTEN. *African Economic History: Internal Development and External Dependency*. Portsmouth, N.H.:

Heinemann or James Currey, London. 1987. Pp. ix, 294. \$22.50.

This survey of African economic history south of the Sahara is a tour de force. Those who know Ralph A. Austen's work have been aware for many years of the omnivorous nature of his reading, and his ability to synthesize vast amounts of material is put to good use in this volume. The result is both theoretically eclectic and scrupulously fair.

Austen's coverage extends from the beginning of food resource domestication to the postcolonial era. More comprehensive geographically than A. G. Hopkins's excellent *Economic History of West Africa* (1973), the volume treats East and South Africa as well.

The canvas is broad, the treatment is intellectually at a high level, and Austen's determined eclecticism is convincing. The description and analysis of Africa's precolonial trade networks and their management by merchant groups are particularly good. He keeps in focus how the various regions of Africa were connected economically with one another, how development proceeded at a different pace in the different regions, and how technology influenced the various economies from a very early period. We are treated to generous helpings of material on the spread of mining and agriculture, the growth of artisans and urban centers, and the often-neglected role of precolonial money and credit. Austen is always alert to the sociological dimensions of economic history, seen most importantly, perhaps, in his treatment of the sexual division of labor.

His assessment of the colonial period is equally well done. It emphasizes that the partition of Africa took place at a time when that continent was becoming economically less important to Europe, not more so. The search for profit cannot, in Austen's thesis, explain the colonial conquests. Imperial ambitions and a chauvinistic nationalism provide parts of the explanation. Precautionary acquisitions to forestall others politically or economically, as a mercantilistic loss of confidence in open markets and free trade took hold, add to the story. Abolitionist sentiment contributed yet another facet.

What sets the volume apart from other economic histories is the attention given to the period of decolonization and the role of the state in postcolonial Africa. In a measured and cautious concluding section, Austen examines why African economies have performed so poorly over the past two decades and, rather pessimistically, links the poor performance to historical causes as well as to the current derogatory view of the economic policies followed for so long by many of the continent's governments.

I have remarkably few cavils. For what will surely be a widely used textbook in African economic history courses, both graduate and undergraduate, the print should have been larger. The bibliographies are grouped at the end of chapters, as Hopkins chose to do in his earlier venture. A comprehensive bibliography at the end is easier to employ when plucking a book from

the shelf to locate a source, and this work will be much used for that purpose. One complaint that the author himself makes, that the book contains too many notes and bibliographical entries, is, however, not justified. The notes and bibliographies are strong and very much up to date, a great many of them from the 1980s.

A wide gulf continues to separate the several schools of thought in African economic history. Formalists, substantivists, and dependency theorists continue their running thirty-year debate, a debate that some of its protagonists mar with misstatements of positions and bellicose language. It is thus unlikely that any one book will satisfy all sides in the controversy. Yet, in my opinion, Austen's work goes further than any book of the last ten years in attempting to draw together the most persuasive arguments of each of the schools into a reasonable and persuasive synthesis. Above all for that, it deserves wide attention.

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ASIA

S. A. M. ADSHEAD. *China in World History*. New York: St. Martin's. 1988. Pp. x, 422. \$37.50.

S. A. M. Adshead intended to write what he calls a "synoptic history of China's relationship to the rest of the World" (p. x) and to trace in detail whatever contribution China may have made to this assumed world order. The author's purpose is to fill the gap in our knowledge of China by tracing the relation between important stages in China's history and the rest of the world (p. ix).

Adshead appears to have been drawn to his topic by the writing of Pierre Teilhard de Chardin, who propagated a "convergence" of human history and gained in Peking a wide circle of admirers. In developing his thesis, Adshead speaks in his introduction of Teilhard de Chardin's speculations, "hard to escape when once absorbed" (p. ix), but, in his final chapter, the author turns against his mentor's attempt to include some of the results of totalitarianism in the "emerging world order." But the vision of "convergence" remains to Adshead "fundamental" (p. 386).

From this point of view, the book is divided into six chapters dealing with China from Han times to the death of Mao Tse-tung. The author assumes four primary civilizations: East Asia, Western Eurasia, pre-Colombian America, and black Africa (p. 1). In practice, however, Adshead describes the interaction between China and the West. Under each chapter, he follows the same subdivision. First, the China of each period is analyzed; then the routes of contact with the outside world are explored (essentially the northern land route, the central land route, the southern sea route, and the far southern sea route); and finally, after America enters the world scene, the Pacific route is added and in recent time the air route. Next Adshead studies what traveled along these routes—people,

goods, techniques, ideas, values, pathologies, institutions, and myths—and finally what contributions the exchanges made to an “emerging world system” (p. x).

This is an ambitious project whose most interesting part is the discussion of contacts between civilizations. For that aspect alone the book is of value. But it needs a map on which to locate the cities and regions that Adshead endlessly describes. Of much less value are the assessments of China. “Paradigmatic, introverted and ethical,” China under Confucius and Lao-tze had—in contrast to Rome—“no doctrine to teach,” “no wisdom to impart” (p. 13), and, although the technical comparisons are valuable, the author misses out completely on the importance of an educated, nonstatus elite, the scholar-gentry, as leaders of society and candidates for office, which the author occasionally describes as a “meritocracy.” The scholar-gentry, a mainstay of imperial China that maintained social and state structure even during the Buddhist golden age under the T’ang dynasty, remains unexplained. Even a few qualifying (and misleading) adjectives, such as the “etatist (p. 112) promoter of laissez faire Wang An-shih” or the “conservative panjandrum Ssu-ma Kuang” and other similar flourishing characteristics, give no picture of the great confrontation between two schools of thought during the Sung dynasty (p. 113). Although the role of tea, silk, porcelain, compass, and printing, among other items, and the issue of written examinations are well recorded, not enough is said of the impact of Chinese concepts on the leaders of the Enlightenment in the West.

The history of contemporary China suffers Adshead’s lack of interest in Marxist-Leninist ideology and Stalinism and his disregard for the physical elimination of the millions of noncommunist upper classes by the Maoist leadership as part of the communist program. As a history of China, the book fails; as a history of the contacts between China and the West, the book merits attention.

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DAVID MCMULLEN. *State and Scholars in T’ang China*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1988. Pp. x, 423. \$49.50.

David McMullen wrote his compact study of the Confucian-oriented scholar elite of the T’ang dynasty to counter the effects of a bias that he traces back to the rise and development of neo-Confucianism. Finding the T’ang Confucians wanting in their understanding of the essential truths of Confucianism, the neo-Confucians disregarded their ideas or at least deemed them less worthy of serious consideration than the ideas of the masters of the classical and neo-Confucian periods. In like manner, modern students of Chinese thought and intellectual history have focused attention on the

classical and neo-Confucian traditions at the expense of the T’ang dynasty and its Confucian legacy. McMullen provides a panoramic view of T’ang Confucianism, featuring the scholars responsible for its “distinctively medieval identity” (p. 250) and those who shed that identity to foreshadow some of the defining characteristics of the neo-Confucian tradition.

McMullen organizes his study around five fields of scholarly endeavor—education, canonical scholarship, state ritual, the writing of history, and literary activity—and analyzes developments in each field as they occurred in the three periods of T’ang social and political history: the reigns of Kao-tsu and T’ai-tsung; the period extending from the accession of Kao-tsung to the eve of the An Lu-shan rebellion (650–755); and the postrebellion period. His research shows that, during the middle period, T’ang scholarship took on a new dimension that in turn yields evidence of a changing climate of opinion in the T’ang, most prominent in the aftermath of the rebellion.

During the founding reigns, the court and agencies of the central bureaucracy dominated scholarship. Office life in the capital was the setting for the pursuit of Confucian learning, and learned principles and ideals were brought to bear on the scholarly fields that McMullen considers by men working in close cooperation with the emperor. Much the same may be said about the scholarship of the century preceding the rebellion with one crucial difference: surviving sources from the period show that scholars also recorded independent attitudes in private writings, a practice that gained momentum after the rebellion when the court and central bureaucracy could no longer dominate scholarship. During the decades following the rebellion, creative scholarship reached its peak in these private writings.

The most important point McMullen makes about the postrebellion period is that increasing numbers of scholars shared in common an independent perspective and a critical attitude toward administration, society, and culture in the broadest sense. Rather than approach issues from perspectives refined in the context of high-level official life in the capital, men were apt to write from a perspective born of personal experience outside the capital, in a scholarly world larger, more varied, and suggestive of richer possibilities than the court-centered scholarly world of Ch’ang-an. Scholars who did this shed the “medieval identity” that characterized most of T’ang scholarship and adumbrated the creativity of their neo-Confucian successors (and, in some cases, crucial neo-Confucian ideas).

This book should be read by those interested in the T’ang dynasty as a discrete period in Chinese history, in Chinese intellectual history in general, and in the story of the unfolding of the Confucian tradition. Regardless of why the book is read, it will go far to complete one’s picture of the T’ang dynasty and its Confucian legacy.

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JAMES L. WATSON and EVELYN S. RAWSKI, editors. *Death Ritual in Late Imperial and Modern China*. (Studies on China, number 8.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xv, 334. \$40.00.

In 1903 the missionary W. Gilbert Walshe wrote that the subject of Chinese funeral ceremonies would not be exhausted for a long time to come, "so varied are the methods which obtain in different parts of the Empire." The authors of this book confirm that the subject is not exhausted but contend that underlying similarities existed beneath the observed variations. Their work should be of considerable interest to Chinese historians and not only because most of the essays concern the past. A generation of Chinese social historians was deeply influenced by the anthropologists of the 1950s, 1960s, and 1970s, who proposed models of Chinese family, kinship, marriage, marketing structures, community organization, and popular religion. This book suggests that, as anthropologists of China follow the general anthropological trend toward analysis of symbols and rituals, the ties of historians and anthropologists will become even closer and more reciprocal.

Two debates run through these essays, giving the volume unusual coherence. The first concerns belief versus performance. In a deliberately provocative introduction, James L. Watson asserts that performance took precedence over belief in imperial China. To the state, in particular, "it mattered little what one believed about death or the afterlife as long as the rites were performed properly" (p. 4). Evelyn S. Rawski counters in a second introduction by arguing that common ideas about Yin and Yang, ghosts and ancestors, salvation and rebirth underlay the uniformities in performance and that the state's concern with people's performances was based on its concern with their moral values. A second debate concerns uniformity and variation. Watson posits the existence of a basic sequence of rites for funerals that included the use of paid professionals, music, airtight coffins, and so on, and argues that state promotion of correct practice (orthopraxy) led to a high level of uniformity in adherence to this sequence. By contrast, final disposal (burial, cremation, exhumation, and reburial of bones) was not a subject of political control and could be used to express social, regional, and ethnic differences.

The ten principal essays in this volume all contribute to these debates. Most confirm the existence of uniformity at some level but differ on how far it extended and the role of the state. Rawski's essay on the funerals of Ming and Ch'ing emperors lends little support to the thesis of a state-established orthopraxy in the handling of the dead, because she sees emperors modeling themselves on commoner practice more than the other way around. Susan Naquin, looking at descriptions of funerals in North China in the nineteenth century, attributes basic uniformity largely to the existence of standard manuals and ritual experts, not the state. Myron Cohen points to state efforts to suppress cults

that emphasized salvation, showing its concern with belief. Elizabeth Johnson provides evidence that paid professionals were not a part of all funerals, even though she did her field work only a few miles from where Watson did his. Martin Whyte challenges the idea that burial was not a part of the standard ritual sequence, as the switch to cremation promoted by the Communist government was widely experienced as a "traumatic violation of the required ritual sequence" (p. 292).

In addition to the issues set out by the editors, gender emerges as a powerful theme linking several essays. Drawing especially on insights from *Death and the Regeneration of Life* (1982), edited by Maurice Bloch and Jonathan Parry, several authors look at death rituals with attention to the symbolism of fertility, connections between fertility and mortality, and gender differentiation in belief and performance. Stuart Thompson provides an astute analysis of the symbolism of food offerings that identifies men with pure white rice and women with red fleshy pork. Johnson shows how women could use their required funeral wailing to express emotions not acceptable in other contexts. Watson notes that men avoid touching corpses, but women do not. Emily Martin suggests that this avoidance is because women, through their actions at funerals, strive to overcome the distinctions of life and death, whereas men strive to separate opposites and resist cyclical change.

The essays in this volume are all intelligent, well written, and well edited. Given the framework set by the editors, an essay making use of religious documents and liturgies or one based on interviews with active Buddhist or Taoist priests would have been a useful addition. The argument also would have benefited from a study of the performances commonly put on at funerals, such as the Mu-lien plays, because such plays are certainly performances but very much involved in representing ideas. Let us hope that this volume will stimulate new research into these areas.

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SHUM KUI-KWONG. *The Chinese Communists' Road to Power: The Anti-Japanese National United Front, 1935-1945*. (East Asian Historical Monographs.) New York: Oxford University Press. 1988. Pp. xii, 312. \$36.00.

Shum Kui-kwong argues that the anti-Japanese national united front policy was the primary factor that promoted the expansion of Chinese Communist power between 1935 and 1945. It was the united front, with the social and economic policies it implied, that generated the public support that forced the Guomindang to conclude a ceasefire with the Chinese Communist party (CCP) at a time when the Communists were weak and vulnerable after their defeat in Jiangxi and the rigors of the Long March. Within the framework of anti-

Japanese resistance, Mao Zedong followed a dual policy. On the one hand, he created rural guerrilla bases and mobilized the peasantry through resistance activities. On the other hand, he promoted a united front policy to gain the support of both urban and rural elites and to isolate the Guomindang. That strategy enabled the Communists to grow during the decade in question, so that by the end of the anti-Japanese war they were in a position to contend successfully for national power.

Some readers will find this thesis familiar when couched in such general terms. It has been suggested in one form or another in a number of works, and Kataoka Tetsuya, to take a notable example, in his book *Resistance and Revolution in China: The Communists and the Second United Front* (1974) argues specifically that "the second united front was of paramount importance for the success of the revolution" (p. 10), a statement that essentially sums up the chief theme of Shum's book. The recent synthesis by Lyman Van Slyke in volume 13 of the *Cambridge History of China* also makes clear the importance of united front policies in providing the opportunities for Communist growth. The differences between Shum's book and the other treatments lie in Shum's insistence that previous works focus too exclusively on the reasons for peasant support of the Communists and not on the equally important fact of the CCP's "united front with the majority of both rural and urban elites" (p. 5). In addition, Shum develops his own ideas about many issues of detail and interpretation regarding events during the decade after 1935 that are still matters of debate among specialists.

In formulating his arguments, Shum is particularly concerned to demonstrate how the Chinese Communists have rewritten, and distorted, history. When the Communists systematically revised the historical record of Liu Shaoqi's career after the Cultural Revolution, Shum wondered if the same thing might have been done with Wang Ming (Chen Shaoyu), the leader of the Russian Returned Students at the beginning of the 1930s. Wang lived in Moscow from 1931 to late 1937, and during that time he was a spokesman for a united front policy, which the Russians strongly advocated. When he returned to China, he continued to press for a comprehensive and genuine united front. Shum tries to show that, in the early days of the united front, there was much less disagreement between Mao and Wang than has generally been thought. Shum is preparing a separate volume about Wang Ming during the Jiangxi period, but much of the analysis in the present volume is concerned to show that Wang Ming's role has been systematically distorted by Chinese Communist historiography under Mao. In his analysis, Shum also notes in passing a host of other distortions in Chinese official history and incidentally suggests that Western historians have too readily accepted the Maoist version of history.

Shum uses Communist materials from major archives in Australia, Hong Kong, Japan, China, and the

United States. His argumentation consists primarily of detailed analysis of documents and materials to infer chronology, authorship, interrelationship with other documents, and motives of individuals and groups. The book will be of particular interest to specialists in Chinese Communist history to whom the details of the party's doctrinal and political struggles are familiar.

JAMES E. SHERIDAN
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MICHIO UMEGAKI. *After the Restoration: The Beginning of Japan's Modern State*. New York: New York University Press. 1988. Pp. xiv, 270. \$35.00.

The author of this monograph constantly reminds us that he is presenting new interpretations and exploring new territory. Leaving aside the first claim for a moment, it is difficult to understand the second. Far from being a "neglected decade" as the author suggests (p. 8), the first decade of the Meiji regime has been reconnoitered by Robert A. Wilson, Nobutake Ike, George Beckmann, Robert Scalapino, George Akita, Andrew Fraser, W. G. Beasley, Conrad Totman, and Harold P. Bolitho. If anything has been neglected, it is the development of a new perspective on the period. The author attempts this but, in my view, does not succeed.

The two questions he addresses are: how and why did the Meiji government succeed in bringing about the abolition of the domains and the establishment of a centralized administrative structure and how did this "bloodless revolution" affect the coalition of political forces that overthrew the Tokugawa *Bakufu*?

Michio Umegaki's argument, briefly stated, is that the abolition of the domains was the result of administrative pressures both at the center and on the periphery rather than "shrewd management of a rational career designed by men" (p. 108). (By the way, that superfluous "designed by men" is typical of the author's style.) Once the domains had been abolished, resolving the fundamental political question of how much of the old order would survive, the restoration coalition (or perhaps the imperial government coalition?) began to fall apart. Each top government leader saw himself as a "proxy of the emperor," and "each asserted his right to prevail over the others in the name of the emperor" (p. 163). The leadership was divided between those who feared that the government was overcentralizing (Saigō Takamori and Itagaki Taisuke) and those who feared that it was rudderless because it was not centralized enough (Ōkubo Toshimichi and Iwakura Takamori). The division was resolved only when members of the first group left the government after the crisis of 1873 and pressed their views by other means, that is, domain-based rebellion or agitation for a more "representative" government.

So what else is new? The outlines of this argument should be familiar to the reader of any standard textbook on modern Japan, let alone to those who have

read the monographic literature. The author tries to clothe the argument in new language. He suggests that two "types of political dynamics" were at work during the period: "the simultaneous dispersion and consolidation of political power" (p. 14). This language is descriptive rather than analytical. The author rarely makes clear in any precise way when and how these "dynamics" influenced political or administrative decisions, and he obscures their interaction by suggesting the two worked against one another. In fact, the post-1871 dispersion of political power was quite different in character from the pre-1871 dispersion, and both served the goal of greater consolidation of authority in Tokyo.

All this is not to say that the book does not make a contribution to our understanding of the postrestoration political consolidation. Although Umegaki offers no new perspectives, he certainly "fills gaps" (to use the orthodontic metaphor) in our knowledge and "sheds light" (to use the electrician's) on obscure topics. He demonstrates the alacrity with which the domains climbed on the imperial bandwagon in early 1868; he describes in detail the process by which the imperial government took over the old *Bakufu* domains; he outlines the financial difficulties that made the domains willing to surrender their administrative powers to the central government; he provides statistical confirmation that the top government leaders were recruited from a very narrow social and regional base; he describes how intensely central government officials were resented by ex-samurai peers back in their former domains; he points up the role of the Finance and Home Affairs departments as "a government within a government"; he suggests that younger officials like Yamagata Aritomo, Inoue Kaoru, Ōkuma Shigenobu, and Itō Hirobumi were the driving force behind reform within those departments; he shows how the *ku* gradually accumulated administrative functions and how they drew local leaders into the political process; he gives a good brief description of Saigō's "caretaker government" of 1872–73; and he argues that the real lesson of 1878 was not that violence no longer worked as a means of opposing the central government but that the old domain no longer provided a vehicle for political change. All of this is fresh, and it provides a useful amplification of the available English literature on the first Meiji decade.

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D. ELEANOR WESTNEY. *Imitation and Innovation: The Transfer of Western Organizational Patterns to Meiji Japan*. Cambridge: Harvard University Press. 1987. Pp. viii, 252.

During the Meiji period (1868–1912), Japan underwent dramatic change as its leaders sought, by using foreign models, to transform their country into a nation capable of dealing on equal terms with the West.

Sociologist D. Eleanor Westney believes that understanding the development of complex organizations is a key to understanding such social change. In this book she examines the development of three Meiji organizations that were based on foreign models: the police, the postal system, and the press. (Westney has already published an analysis of the military in Marius B. Jansen and Gilbert Rozman, eds., *Japan in Transition: From Tokugawa to Meiji* [1986], pp. 168–94.) She asks six questions about each organization: why the specific model was chosen; which features of the model were observed and which selected; what the reasons were for departures from the model; how resources for the new organization were secured; whether the new organization became more like or less like the original model over time; and in what ways the new organization affected the social environment and other organizations.

Westney answers her questions with abundant and fascinating detail, detail all the more interesting because the police and the postal system have received almost no treatment in Western languages and the coverage of the press has been confined mostly to its political role. The complexity of the transfer process and the number of creative adaptations that occurred give point to Westney's remark, "the successful imitation of foreign organizational patterns requires innovation" (p. 6). Especially noteworthy are the occasions when the Japanese pushed ahead of the Western model, for instance, the introduction of formal police training and the professionalization of Japanese police prior to such developments in the West. Although the model for those changes may have been, as Westney suggests, the Westernized Meiji army, it is also possible to see at work earlier Japanese traditions. The Tokugawa samurai met many of the standards of professionalism, and it was not unusual in pre-Meiji times to establish schools for training in special areas of expertise, for example, the Chōshū domain's Naval Instruction School.

One of my concerns about this book is the narrow range of materials on which the studies of the police and the postal system are based. As she must, Westney relies heavily on official and semiofficial histories of those organizations, but such histories tend to be focused on and to enlarge the role of the actors who belong to the organization. The book would have been more balanced if Westney had broadened the scope of her research and, among other things, given us more information about how decisions affecting the organizations were worked out among the top leaders of the Meiji government. Those men controlled the destinies of the organizations but are almost entirely absent from Westney's account.

In her conclusion Westney discusses the problem of convergence and divergence. Japanese organizations that drew their inspiration from Western models display many differences from analogous Western organizations. Why is it that over the years the common imperatives of complex and large-scale organizations

required for industrialization and state building have not produced greater convergence of form? Part of the answer lies, Westney would agree, in the "late development effects" described by Ronald Dore and others, for example, that the later the development begins, the greater the direct role of the state in economic organization. Culture, a commonly offered explanation, Westney does not see as a primary cause of the differences. The three organizations under discussion show, she says, "a reshaping of the Japanese traditions to fit the needs of the organizations rather than a reshaping of the Western models to fit the traditions of Japan" (p. 219). She finds a much better approach in two perspectives coming from organizational sociology: the "organization as organism" and the "organization as a social actor" (p. 222). The organization-as-organism perspective, for example, asserts that the form that prevails will be the form most efficient in getting and using the resources in its immediate environment. If the resource configurations in two societies are very similar, there will be a pull toward convergence; if different, a pull toward divergence. Those theoretical approaches offer new insights, but their full value goes unrealized because Westney does not integrate them explicitly and fully into the presentation of her cases.

Written in clear and accessible language, this book is a pleasure to read. It will be of substantial interest and value to all who study Japan and to anyone concerned with cross-societal transfer. It will also, I hope, be a model whose imitation will lead to innovation in Japanese studies.

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SHIREEN MOOSVI. *The Economy of the Mughal Empire, c. 1595: A Statistical Study*. New York: Oxford University Press and Centre of Advanced Study in History, Aligarh, India. 1987. Pp. xi, 442. \$29.95.

In this impressive study, Shireen Moosvi's quantitative estimates delineate the economy of Mughal India in the last years of Akbar's reign. Moosvi has closely analyzed the statistical data contained in Abul Fazl's *A'in-i Akbari*, the great manual of the empire. She has collated these data with the *Akbar-Nama*, Abul Fazl's chronicle of the reign, and with any other extant quantitative Mughal or European sources. Reconciling and establishing uniform tabular formats for these varied materials from manuscripts as well as published works was clearly a formidable task. Clarifying the discrepancies in Abul Fazl's statistics is a much-needed contribution in itself. For long-term trends, Moosvi turned to similar statistical data compiled in colonial India in the late nineteenth and early twentieth centuries.

Moosvi is strongest in her detailed analysis of the Mughal administrative data in which she portrays a huge, efficient, and profitable imperial system. She concludes that the Mughal administration obtained approximately 60 percent of the total land revenue

assessment (*jama'*) demanded (p. 193). Some 20 percent went to costs and the local aristocracy; the zamindars received, on average, about 20 percent of the total revenues extracted from the countryside (p. 186). From these revenues Akbar accumulated an enormous treasure. Moosvi's computations for the Mughal budget reveal expenses divided between the imperial household and central military (14 percent combined) and the annual pay bill for *mansabdars* (82 percent). She confirms that Akbar easily met his annual expenses and set aside a comfortable 4 percent surplus on the massive Mughal revenues every year. Timurid wealth had direct implications for the monetary system, which attracted increasing supplies of New World silver and gold. Moosvi employs numismatic data to track the huge increase in silver currency produced by Mughal mints in the period from 1556 to 1630. Despite this influx, Moosvi argues that prices remained stable until after Akbar's death in 1605 (p. 371).

Fascinating, but more speculative, are her conclusions regarding long-term trends. In agriculture, the gross area under cultivation in Akbar's India was about half that in the early twentieth century (p. 65). Average food-grain crop yields and "overall productivity remained broadly stationary or . . . marginally declined" between 1600 and 1900 (p. 91). Conversely, using price and wage data in the *A'in*, Moosvi concludes that real urban wages were higher in 1600 than in the last quarter of the nineteenth century (p. 347). Relative Mughal rural and urban taxation suggests that the ratio of rural to urban population was about 15 percent of the total population in 1600 (p. 305) but that the ratio of urban population declined considerably in the next three centuries.

Moosvi has constructed a firm base line from which to assess the Mughal empire's political economy. Her systematic analyses and formulas are clearly explained and argued. She has made ingenious use of her experience with the Mughal historical atlas to map these difficult data and show regional differences. Her arguments on long-term trends are stimulating and much appreciated. Further research on the seventeenth and especially the eighteenth centuries will support or disprove these generalizations. In either event, we must be grateful to Moosvi for her breadth of vision and her careful scholarship.

JOHN F. RICHARDS
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AMIYA KUMAR BAGCHI. *The Evolution of the State Bank of India: The Roots, 1806-1876*. Part 1, *The Early Years, 1806-1860*; part 2, *Diversity and Regrouping, 1860-1876*. New York: Oxford University Press. 1987. Pp. xxii, 535; xii, 515. \$45.00 the set.

The one thousand or so pages contained in these two volumes are devoted to a detailed survey of the development of the progenitors of the State Bank of India—the so-called three Presidency Banks, namely, the Bank

of Bengal, the Bank of Bombay, and the Bank of Madras—from their respective foundations in 1806, 1840, and 1843 until the passage of the Presidency Banks' Act in 1876. Part 1 is largely concerned with the Bank of Bengal and its forerunner, the Bank of Calcutta, whereas at the center of Part 2 is the stormy history of the Bank of Bombay during the mid-nineteenth century. The division of the two volumes stems from the Mutiny of 1857–58, or India's first war of independence, which led in 1862 to the replacement of these banks' own private notes by government currency notes.

This study is very much an institutional business history, based substantially on the banks' own surviving records. Here Amiya Kumar Bagchi has been well served because the only major parts of the historic archive that have been lost are the directors' minutes and the branch records of the (old) Bank of Bombay for the period 1840–67. Indeed, the banks themselves had a sense of history from 1837, when the Bank of Bengal began to build up a collection of material on its own development and Indian banking in general, and, subsequently, two of its senior officers wrote what came to be standard works (C. N. Cooke, *The Rise, Progress, and Present Condition of Banking in India* [1863] and G. P. Symes Scutt, *The History of the Bank of Bengal* [1904]). They were, of course, accounts with a European focus, and Bagchi's aim is to write a history from an Indian point of view with the development and changing functions of the banks set against the wider socioeconomic background.

The distinctive characteristics of the Presidency Banks were that they were chartered institutions with government shareholdings and government-nominated directors. The banks arose precisely from the prior conditions of the East India Company for the provision of charters. Further, they held substantial government balances and, until 1862, issued bank notes. They conducted what Bagchi has termed "apex banking," their business being largely with substantial or long-established merchants, generally Europeans, or involving investment in government securities. Bank business was generally closely controlled by the stipulations of the banks' various charters, and one major area, exchange banking, was forbidden. The Bank of Bengal arose out of the financial consequences of warfare for the British governance of India. Private, nongovernment business did not develop substantially for the bank until 1817; rather, in its early years the bank improved the government's credit standing, which led to a fall in interest rates, through the replacement of the government's depreciated treasury bills and notes by the bank's own notes. The market in East India Company paper also steadied because such securities were the predominant collateral for the bank's loans. In Bombay the same pressures from state finance did not occur, so the port's local mercantile community used the Bombay treasury as a deposit bank from 1812 until 1836. And a government bank was founded in Madras in 1806. A trade boom in

Bombay in the 1830s, following the end of the East India Company's monopoly of trade to China in 1833, led to moves from middle-ranking trading houses to establish a bank modeled on the Bank of Bengal, possibly to defeat a London proposal for a general bank of India. Local agency houses in Madras rapidly followed the lead given by Bombay.

Despite being state-backed institutions, the Presidency Banks did not have their own way in Anglo-Indian banking. During the 1830s and 1840s, the Bank of Bengal had to compete with the Union Bank. Beginning in 1833 the up-country banks emerged, led by the Agra Bank, and, although the proposal for a general bank of India came to nought, the exchange banks, on the basis of London backing, were established, beginning with what became the Oriental Bank in 1842. Bagchi merely acknowledges these evolutionary steps in the development of Anglo-Indian banking, and he considers new entrants in Anglo-Indian banking only when they directly impinged in a substantial way on the activities of the Presidency Banks, for example, the competition between the Union Bank and the Bank of Bengal and the rivalry in the 1860s for treasury business between the Agra Bank and the Bank of Bengal. Bagchi has constructed an immensely detailed account of the particular business of the Presidency Banks to the extent that it is a grass-roots view with often lengthy citation of primary material both in the text and in appendixes to chapters. Consequently, the reader is frequently overwhelmed, and I was grateful for the system of headings within the chapters that provided a guide to at least the progression of the material. The overall effect will be daunting for the general reader who needs to know something about Anglo-Indian banking.

Non-specialists will find few introductory generalizations and summaries of arguments. All too frequently they will be confronted with a quarry of almost raw material from which their own conclusions will have to be constructed, and few blueprints are provided. The specialist will encounter a treasure trove that can enrich personal research. I found particularly informative the view of the agency house crisis in Calcutta during the decade after 1825 as seen from the minutes of the Bank of Bengal, as well as the account of the way in which the (old) Bank of Bombay undermined itself during the speculative and feverish bull market in cotton in the mid-1860s. I suspect, however, that very few readers will have sufficient concentration to read both parts in their entirety, and those for whom this work will be mandatory reading will have to develop a strategy by which to approach it.

Is this an Indian view of a major aspect of the development of Anglo-Indian banking? Certainly, and the continued cultural bruising arising from an imperial past may be responsible for some of the stridence in the exposition. Nonetheless, the author's own accumulation of evidence points to some amelioration of parts of the commentary. Indian firms and Indian associates of the European firms that banked with the Presidency

Banks appear to have had more resilience than is suggested here. Certainly they were affected by crises, and certainly the banks may have been less generous to the Indian mercantile community than they were to Europeans during periods of stringency. Many would appear, however, to have been like bamboos, able to bend before a prevailing wind but then spring back again. The author has interpreted his remit very narrowly; there is only one general survey chapter in each part. Because the banks were products of the Anglo-Indian community and because it was that community that substantially constituted the banks' clientele, the banks did affect to some degree the well-developed indigenous banking system. As Bagchi points out, as early as 1817 loans by the Bank of Bengal were taken up by the Calcutta bazaar and lent again. Such connections are, however, only considered in passing, so the wider influence and penetration of these banks is difficult to assess. Similarly, the workings of internal exchanges arising out of regional markets and inter-regional transfers are only occasionally discussed in some depth, for example, the effect when the banks secured residents' treasury business in the princely states and when the Bank of Bengal in Bombay established an agency in Bombay. This particularly narrow definition of a business historian's remit has probably resulted in some lost opportunities for an Indian view of the further commercialization and monetization of the subcontinent during the first three-quarters of the nineteenth century.

P. L. COTTRELL
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J. ROYAL ROSEBERRY III. *Imperial Rule in Punjab: The Conquest and Administration of Multan, 1818–1881*. Riverdale, Md.: Riverdale. 1987. Pp. 285. \$35.00.

J. Royal Roseberry III brings us a study of the past that seems to be of that past. The subjects chosen for discussion, the concepts and stereotypes that appear, and even the vocabulary are those of the British-Indian empire. The work creates the impression that one is reading a document from another era, an impression that is strengthened by a generous use of quotations. In addition, the author seems to be unable to step beyond the world of the colonial official and add a degree of historical perspective expected of a trained scholar. The sources used here contribute to the vision presented. They are almost exclusively from British government records, standard histories, letters from one official to another, diaries, and memoirs. There is only a handful of citations from the works of contemporary South Asian historians and a complete lack of Urdu literature produced by the citizens of Multan. This deficiency is most apparent in the last chapter, which deals with communal conflict in the 1880s. An exploration of indigenous literature would have broadened this study beyond the world as seen through British eyes and British perceptions. There is as well a lack of

any analytical structure that might help place the events discussed in perspective.

This book is a descriptive political-administrative history. The author begins his fourteen chapters with an introduction followed by a discussion of the administrations of Diwan Sawan Mal and his son Diwan Mulraj, who governed semi-independently within the Sikh kingdom. The revolt of 1848–49 and Multan's conquest by the British are examined next. Then the author devotes two chapters to initial British administrative efforts with an emphasis on land revenue and irrigation. Chapter nine deals with the mutiny of 1857 and is followed by two chapters that again examine issues of administration, particularly land revenue and the judiciary. The next two chapters focus on the acquisition of peasant land by money lenders and on questions concerning the land tax. Finally, there is a chapter on the communal conflicts of 1880–81 and a five-page conclusion that deals only with the preceding chapter. There is no attempt to draw together the entire volume nor to relate events in Multan to the broader world of imperial history.

The reader is left with a final thought in relation to the question of communal relations: "To close on a less somber note, there might be some comfort for the world in the alacrity with which the rank and file of riot-torn Multan slipped back into their old pattern of live and let live. This could indicate that when enough is riding on it peaceful coexistence becomes habit forming like almost everything else" (p. 274). This volume adds little to our knowledge of nineteenth-century Punjab. It does present a coherent recital of Multan's administrative and political history with some discussion of social groupings and communal relations. For individuals who wish an introduction to the Multan's history from 1818 to 1881, this study will provide it, but beyond that its uses are limited.

KENNETH W. JONES
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IMRAN ALI. *The Punjab under Imperialism, 1885–1947*. Princeton: Princeton University Press. 1988. Pp. ix, 264. \$49.50.

This book is a carefully documented study of the development and impact of canal irrigation during British rule in what is now Pakistan. Between 1885 and the creation of Pakistan in 1947, about eleven million acres in the great stretch of territory between the Beas and Satlej rivers and the Jhelum were supplied with water from the rivers, making it one of the most productive agricultural regions of northern India. Irrigation had, of course, been common in other parts of the Indian subcontinent throughout history, but almost always it had been a process of bringing water to already settled land with the intention of improving existing agriculture and supplementing the monsoon rainfall. The situation in western Punjab was quite different. The areas between the rivers, known as *doabs*,

received little rain, and, because through the centuries the action of the rivers had made them much higher than the river beds, small-scale irrigation was not possible. Much of the region was largely uncultivated wasteland, sparsely inhabited by a seminomadic population of cattle and camel graziers. After water was made available through the vast system of canals from the rivers, the land was brought under cultivation, not by the local people but by colonists from elsewhere in the Punjab who were encouraged, through various government schemes, to settle in the area. The "canal colonies," as they were known, were of great importance to the British in the Punjab, as they are to their successors, the Pakistanis, and beyond this, Imran Ali argues, their impact on the area provides insights into how a region could undergo significant economic growth and not only remain backward but also acquire "through the very process of growth further structural resistances to growth" (p. vi).

This is an important and intriguing concept, but, unfortunately, the author's treatment of it is flawed by simplistic notions about the nature of power and the political process. He appears to believe not only that the British wanted to change the social and economic structure of India but also that they should have done so and could have if they had not been anxious to maintain their power and fearful that change might have undermined their rule. Such contradictions in historical understanding surface frequently, but they do not prevent the presentation of a remarkably lucid account of the interplay of government policy and private interest.

The nine canal colonies were created over a period of forty years (although the last, Nili Bar, was not completed until the 1940s) with methods of colonization that differed from area to area, but one of the values of Ali's book is that, although he indicates these differences, he is able to explicate the general trends and patterns of settlement. Some of the land was sold at auctions; some was given for military service; some went as awards for meritorious service in the bureaucracy; and some grants went to great families who had proved their loyalty to the government during the 1857 uprising. A spectacular example was the grant of nearly eight thousand acres in one colony to Sir Khem Singh Bedi, but lesser grants assured the continuing loyalty of the old landed elites. Ali at times gives the impression that this policy was a departure, but it was, of course, the practice that had been followed by the Mughals, the predecessors of the British. Ali makes the interesting point that this process of what he calls "entrenchment" had great significance for Indian political history in the twentieth century. In the canal colonies, he argues, the state became more deeply entrenched in economic life than it did elsewhere in India, while at the same time the owners of the newly settled lands regarded the British as their benefactors. In other areas of India, he argues, the bourgeoisie became deeply involved in the nationalist movement through their great political organizations, the Indian

National Congress and the Muslim League, because they were not economically or socially dependent on the state as were their counterparts in Punjab. He cites as examples the support of the government by the great Nun, Tiwana, and Daulatana families and their opposition to nationalist politics, whether of the congress or league variety, as the result of canal colony land grants. There is no doubt a great deal of truth in this view, but again Ali overemphasizes the power of the British and does not recognize the special nature of the landowners in the region in contrast to other areas of India.

It is a matter of regret that a book that probes, with such energy and learning, the relationship and attitudes of various groups—peasants, landless laborers, servicemen, and, above all, the landed elites—to the imperial power depends so exclusively on its official records. This is, admittedly, a common characteristic of works on modern South Asia, even by the so-called subaltern historians who seek to show the linkages between activity and consciousness, and, because Ali has focused his study largely on government policy, this use of official sources can perhaps be justified. Surely at least some of the landed families, however, have documents and papers that might demonstrate the reality behind Ali's reading of their role, even though we are forced to see the rest of the population refracted through official documents.

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TAPAN RAYCHAUDHURI. *Europe Reconsidered: Perceptions of the West in Nineteenth-Century Bengal*. Delhi: Oxford University Press. 1988. Pp. xviii, 369.

This book discusses how European civilization was perceived by three leading late nineteenth-century Bengali intellectuals. The three are Bhudev Mukhopadhyay (1827–1894), an orthodox Hindu and a prolific essayist; Bankimchandra Chattopadhyay (1838–1894), the foremost Bengali novelist of his day; and Swami Vivekananda (1863–1902), the Hindu missionary to the West. Why choose these three instead of three others? According to Tapan Raychaudhuri, their works are informed, sophisticated, and perceptive; the three were similar in background but quite different in their evaluations of Western civilization; and they were highly influential among their countrymen.

Among Asians, Bengalis had the longest and most intimate encounter with the West. In the nineteenth century that encounter led to creative syntheses in religion, the arts, and political and social attitudes that prepared the ground for Indian nationhood. Raychaudhuri begins with a succinct intellectual history of nineteenth-century Bengal. He then devotes a lengthy chapter to each of his three thinkers. Each chapter includes a biographical sketch and a brief discussion of the thinker's major writings on the West and the world.

Bhudev, who came from a well-adjusted and loving orthodox Brahmin family, genuinely believed in the superiority of Hindu culture and was highly critical of those Bengalis who blindly imitated the British. He considered the harmonious, hierarchical Indian society healthier than that of the strife-ridden West with its ideas of liberty and equality. From the West, India could learn only practical skills.

Bankim, the most complex of the three, felt exploited by the British, whom he served as a deputy magistrate. He derived his attitudes toward the West in large part from unpleasant experiences with Britons in India. His prescription for the weakness of his countrymen was discipline and self-strengthening, and he wrote about the West to teach his countrymen why Europe was successful and India a failure. He attributed Europe's success to materialism, the quest for power, and equality of opportunity. His early writings say nothing good about India, but later he reverses his position, stresses the superiority of Hinduism because of its morality and high ideals, and cites the greed and materialism of Europe as evidence of its immaturity.

Vivekananda, the only non-Brahmin, is the most attractive of the three and the only one who actually visited the West. He was the original Hindu sage in the United States and attracted many devoted followers among Americans seeking spiritual guidance. Except for his admiration for Western science, he harbored no doubts that Indian civilization was superior and that India had been and would continue to be the spiritual teacher to the world.

Although the three men were enormously well read, much of what they wrote about European history and society was as biased, distorted, and mythologized as was Western popular writing about Asia at the time. They were prisoners of the racial stereotypes, organic social theories, and other popular pseudoscientific concepts of their day and were the counterparts of those "Orientalists" who used the study of other cultures to promote their own.

Raychaudhuri does not offer us any striking new ideas, but he does augment our knowledge and make us more aware of the power of indigenous culture, as opposed to Western education and influences, in shaping Bengali nationalism. The book is useful for its presentation of the Bengali-language writings of each man on the larger issues of world history and for its three vivid biographical sketches, each of which includes an analysis of the subject's family dynamics, education, economic situation, and temperament. On the negative side, dividing each chapter into "life" and "writings" has its cost: the occasion for a particular piece of writing is seldom mentioned, a sense of the writer's intellectual growth is lacking, and there are repetitious passages. In addition, Raychaudhuri loses an opportunity to examine the cultural context, and hence the deeper meaning, of what is said—and un-

said. Nevertheless, he has written a rich and fascinating book.

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RICHARD SISSON and STANLEY WOLPERT, editors. *Congress and Indian Nationalism: The Pre-Independence Phase*. Berkeley and Los Angeles: University of California Press. 1988. Pp. x, 420. \$48.00.

"The story of India's National Congress is in great measure thus the history of modern India since the close of the nineteenth century," writes Stanley Wolpert early in this volume (p. 22). One is thus led to expect another exercise in mainstream history, that is, the view from the top, history from the winners' point of view. The Congress is practically coterminous with modern India, and its way has been the true path. The dissidents and deviants who strayed from it became lost in the outer darkness of historical irrelevance. Sure enough, one finds in Wolpert's essay all of the familiar villains: the ruling British, by turns imperious and devious; their "feudal allies" (p. 34), the princes and the great landlords; and, of course, the obstinate and demagogic M. A. Jinnah and his Muslim League.

But as the reader moves further into these essays, which Wolpert edited with Richard Sisson, a different story emerges. The essays make clear that at times it was the Congress that was irrelevant (as with the Hindu revivalism of the late nineteenth century) or indifferent (as with the Bengal peasant protests against the Union Boards) or only sporadically interested (as with the problems of the peoples of the princely states). In the case of the peasant agitations in Awad, the Congress was not even aware of the unrest. The Congress's response to the Bengal *swadeshi* movement was a civil war within its own ranks in 1906–07.

Sometimes the Congress was even a hindrance to social progress. The Bombay labor unions went on strike as a part of Mahatma Gandhi's noncooperation movement of 1921 and felt betrayed when he suddenly canceled it. S. Bhattacharya indicates in his essay that the political needs of the Congress never meshed with the economic needs of the unions. For all the talk about the need to help the untouchables, the Congress did practically nothing until the eve of independence, according to Eleanor Zelliott. In her article, she reminds us that B. R. Ambedkar, who quit the Congress in 1942 to join the British viceroy's wartime cabinet, then obtained the first "affirmative action" program, an 8 percent quota of the jobs in government service for the untouchables.

One of the most thoughtful and valuable essays in this collection is the one by Claude Markovits on the wildly shifting relationship between Indian businessmen and the Congress. India's present "mixed economy" came about when Indian industrialists, notably J. R. D. Tata, recognized the growing political pressure

for socialism and headed off any revolutionary seizure of their properties by offering in 1944 to help the state develop its own enterprises. Markovits concludes that India, instead of following the pattern of European ideological developments, reverted to its precolonial pattern, in which leading merchants never held political power themselves but influenced and assisted the ruler, who in turn protected their interests.

Rajat Kanta Ray traces the origins of the Bengal revolutionary movement, which preceded rather than resulted from the partition of 1905. Hindu revivalism and cultural pride sometimes found expression in terrorist violence, and even bogus *ashrams* were opened to shield revolutionaries. Police intelligence reports also told of a seance in which the spirit of Ramakrishna Paramahansa was invoked by some disciples. The youths reported that Ramakrishna, who in life had been the model of a benevolent and kindly saint, now advised them to learn how to make guns and bombs.

Several authors examine the controversial events surrounding the elections of 1937 in the United Provinces (now Uttar Pradesh), a large and important state of North India. The Congress, euphoric after a surprising victory, launched a clumsy drive to attract Muslim members and tried to end the political role of the Muslim League. The action succeeded only in polarizing the two religious communities and propelling the angry league toward advocacy of a separate nation of Pakistan.

Most of these articles began as papers delivered at a conference at the University of California, Los Angeles, in 1984, as the Congress approached its centennial. They maintain a high scholarly standard and bristle with references to letters, memorandums, documents, census data, and other primary sources. Most of the authors are known from their previous publications, but Sisson and Wolpert have usefully gathered into one handy volume the contributors' recent research (or perhaps recent fruits of earlier research).

The contributors to this volume have not shown that the Congress equals modern India or even that the Congress equals Indian nationalism. Indian history is too complicated for such equations, which is what makes it so fascinating and so challenging.

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DAVID HAMER. *The New Zealand Liberals: The Years of Power, 1891-1912*. Auckland: Auckland University Press; distributed by Oxford University Press, New York. 1988. Pp. 418. \$45.00.

This book is required reading for all specialists on the British empire, but it is not hammock literature to be consumed quickly on a summer afternoon. Nor is it fare for the tyro. In this study, which is something of an antidote to William Pember Reeves's *The Long White Cloud* (more recently dubbed *The Long Pink Cloud*), published in 1898, David Hamer has given us a

thoughtful analysis of New Zealand's Liberal party, which governed from January 1891 to July 1912. This government brought to New Zealand compulsory arbitration, women's suffrage, old-age pensions, land for settlers, and credit to keep them on it. To a great extent it heralded contemporary non-Marxian socialism and brought party government and parliamentary democracy to New Zealand. Absent the usual personal papers left by politicians, Hamer's study is largely founded on official publications and stump speeches. He uses his sources critically.

One of the most striking chapters has to do with the dominance of the boisterous and aggressive Richard John "King Dick" Seddon. Seddon was a consummate politician who flaunted his plebeian origins and lack of education to gather power almost exclusively in his own hands. Hamer even suspects that Seddon's tantrums and vulgarity were devices to promote his own ends. At any rate, the old, highly educated elite were discredited by the depression of the 1880s, and the new politicians were self-consciously democratic and nationalist. Probably the most outstanding feature of the new dispensation was that the more urban a community was, the more likely it was to vote Liberal.

The Liberals became not so much a party in a party system as "an ideology of support and justification for a one party regime" (p. 218). In the years of decline, 1909-11, Joseph Ward lived in the shadow of Seddon. Often absent overseas, Ward lost touch with his party and allowed the machine to rust away to uselessness.

Despite Reeves's influence on generations of scholars, Hamer feels that the Liberal ascendancy was not inevitable; rather, he has made a Herculean effort to explain the elements involved. Presumably there might have been a number of other outcomes given the variety of themes and tendencies present. The reader is made aware of how complex the background was. And the author is to be complimented for not insisting on any kind of simplistic formula, nor is he primarily concerned with attacking previous theses. A most valuable addition is the bibliographical essay at the end. I would suggest that all historians of the British empire include this volume in their personal libraries.

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UNITED STATES

LAWRENCE W. LEVINE. *Highbrow/Lowbrow: The Emergence of Cultural Hierarchy in America*. (The William E. Massey, Sr., Lectures in the History of American Civilization.) Cambridge: Harvard University Press. 1988. Pp. xii, 306. \$25.00.

While he was doing research for his wonderful book on black culture, Lawrence W. Levine tells us, he was quite struck by the elaborate parodies of Shakespeare he found in the scripts of antebellum minstrel shows. Such satire connoted a widespread popular knowledge of the Bard whom now we view as a difficult literary demigod.

Pursuing his insight in this book, Levine analyzes the manner in which, during the course of the nineteenth century, the audience for theater, opera, symphonic music, and painting narrowed from broad, diverse, and democratic to exclusive and hierarchical. The image of Edwin Booth, the greatest English actor of the 1850s, playing Hamlet and Lear in tents to the miners of Virginia City and Sacramento strikes us as unimaginable, but it was so. Such audiences were plural rather than homogeneous, Levine stresses: different social strata gathered to enjoy the same event, although this does not mean that they all wanted the same message. Varying tastes were accommodated within the same performance. Farce, animal acts, and popular music could all be on the same bill as Shakespeare. During a concert, Jenny Lind would sing the popular songs of the day in the same *bel canto* style that she brought to operatic arias. Audiences loved melodrama and grand rhetoric, vivid heroes and villains, all of which they could gather from Shakespeare and to which they responded, often loudly, sometimes violently.

During the latter half of the nineteenth century, this heterogeneous and lively shared culture diminished. Booking agents and theater owners created separate facilities for different sorts of culture. Vaudeville split from Shakespeare, popular music from symphonic presentation. Aesthetic and spiritual elevation—sacralization—displaced entertainment. By the turn of the twentieth century, highbrow and lowbrow became standard descriptions of a culture now split and rigid where once it had been fluid and shared.

The high priests of the newly sacralized culture were, Levine stresses, deeply ambivalent about democracy and open-ended process. Shocked by the threatening cacophony of late nineteenth-century urban culture, they wished both to proselytize the masses and to create a refuge from them. Order and culture became synonyms of elitist hopes in a discordant society. Allan Bloom, among others, is but the most recent resident of this cultural "carapace impervious to assault" from below (p. 227). While professing that their defense of high culture will trickle down from the high ground, such elitists gain even greater comfort from maintaining their distance from a popular culture they despise and fear.

Much of the pleasure in reading this book derives from the affirmative relish with which Levine describes the fluid culture of the early nineteenth century. He is openly nostalgic for a time prior to the ascendancy of snobbish elites. Thus, I feel like a Gloomy Gus when I wonder about the equally broad antebellum appeal of public hangings and nativist oratory, to mention two other important public rituals. Analyzing such events might lead us to look toward a broader definition of culture than one that concentrates on the fine arts.

Consideration of overtly political rhetoric could also serve to broaden Levine's treatment of culture. Ever since 1840, when the Whigs took to the hills in pursuit of votes, as Richard Hofstadter long ago put it, or perhaps since the Federalists sold their elitist Constitu-

tion by adapting the popular language of the Antifederalists, American elites have spoken duplicitously of, to, and for "the people." Highbrow claims to superiority have been but one part of the language and behaviors of elites. Pandering and genuine fellow feeling have also remained central, as has a very uneasy relationship with Big Money. Levine has included ambivalence in his model, but his overall thrust describes the rise of the snob. This conclusion suggests the triumph of an antidemocratic elite where others might see a shifting, messy, contradictory, and frequently losing set of claims forwarded by marginal and insecure would-be cultural leaders.

This book, like all of Levine's work, invites us out to play. His writing is highly engaging, his argumentativeness provocative. Even in his lament he gives us hope, for he has written a high-minded and very American defense of the unforeclosed and pluralist potential of democratic culture.

MICHAEL FELLMAN
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RUSSELL SANJEK. *American Popular Music and Its Business: The First Four Hundred Years*. Volume 1: *The Beginning to 1790*; volume 2: *From 1790 to 1909*; volume 3: *From 1900 to 1984*. New York: Oxford University Press. 1988. Pp. xvi, 469; vi, 462; vi, 734. \$45.00 each.

Historians have given much study to American music as an art, but the business side has been neglected. Perhaps seeking vicarious identification with creative geniuses, historians regard entrepreneurial components of artistic life to be beneath them. Moreover, when business dealings are studied, the lives of creative artists either become dull or reveal dark sides of their characters. So the business of the arts seems unsavory to the cultural historian and peripheral to the historian of business.

The entrepreneurial side of the arts, however, has played a significant role in their development and is definitely part of American business history. Until now we have not had a comprehensive history of the business of American music. Russell Sanjek's encyclopedic volumes fill that void. Sanjek, who died just at the completion of his work, had unusual expertise. Neither a trained musician nor a historian, he was a businessman who knew every facet of the music world. For forty years he helped run Broadcast Music, Inc., and served on advisory committees for such bodies as Lincoln Center, the National Endowment for the Arts jazz panel, and the Country Music Hall of Fame. Sanjek's own musical loves were jazz and country, but he did not restrict his three volumes to his own preferences. He covered it all.

At times Sanjek's work is encyclopedic in presentation as well as in scope, his coverage more narrative than analytical. It is lively reading but often conveys facts and historical background to the amplification of no apparent thesis about the music business. Sanjek's

tendency to launch into a narrative independent of music is evident in volume 1, which examines concert and sheet music production from sixteenth-century England through eighteenth-century America. Here, some pages treat Puritanism or colonial Virginia without reference to music. When covering music Sanjek presents fascinating material on the cultural life of England under Henry VIII and Elizabeth I and on the growth of English musical theater in the late seventeenth century. He draws from published sources, so specialists on Orlando Gibbons or Henry Purcell will not find much new in Sanjek's coverage of Elizabethan or Restoration times. They will, however, be impressed with all other sections of the volume and find the published literature in their own field lucidly synthesized. Sanjek does indulge a bit of presentism when drawing parallels to modern figures. Can one really cast gavotte dancers of fifteenth-century Oxford as the Bing Crosby or Benny Goodman of their day? No matter; it is Sanjek's way of bringing his history to life. Music should be fun, and so should any book about music.

Much of Sanjek's detail on England does not directly serve the volumes' overall focus on American music. Sanjek does not systematically trace any Atlantic transference of musical art except through individuals in the English music business who happened to emigrate. Generally, Sanjek holds an implicit Turnerian view that musical institutions in early America evolved more out of conditions here than from antecedents in Europe. Valid or not, that view leaves Sanjek's coverage of English events little apparent purpose with respect to subsequent discussion of America.

Historians of the revolution will enjoy Sanjek's treatment of such figures as Daniel Read, William Billings, and Francis Hopkinson. His feel for the music business resonates through discussions that enliven the struggles of artists within the tenuous economy of late eighteenth-century America. Given the interest of some historians in viewing society from the bottom up, Sanjek's pages on William Billings would make excellent reading for seminars. As in the sections on England, Sanjek couples his discussions of music to narratives of events during the revolution but makes no thematic connections.

In volume 2 Sanjek covers the nineteenth century and draws history and music together more convincingly. The concert and stage traditions in the nation's cities come alive in his discussions of the scrambling for audiences and publicity, the utterly corrupt reviewing press of New York, and the careers of such colorful figures as P. T. Barnum and Louis Moreau Gottschalk. Sanjek casts Stephen Foster's sad life as an extension of the impoverished state of music publishing in the West, where business success was rare. Concerning "The Singin'est War," some have speculated that the North's many songs stemmed from a need to maintain spirits, whereas the South, convinced of its cause, needed but a few songs. Sanjek, however, contrasts the unlimited access to paper, ink, and metal (for plates and type) of

northern publishers to the plight of their impoverished southern counterparts, a more down-to-earth explanation.

Concerning concert life in late nineteenth-century America, Sanjek avoids much discussion of the symphony halls and opera houses. Carnegie Hall, Higginson's Boston Symphony, and others have indeed been well covered. When he touches such subjects, Sanjek's tone is actually a trifle hostile. Sanjek dwells instead on the efflorescence of black music, from spirituals and minstrelsy to ragtime, and on the genres of "popular" music that merged with new technologies in the late nineteenth century to produce the plethora of modern music and stagings. The businesses that organized the innovations of recording and amplification were key forces in shaping the course of musical developments. Cultural historians can profit here, lest they trace artistic change from purely aesthetic factors.

The interaction of the businessmen who provided the organization for the composers and performers is the essence of Sanjek's discussion of the modern era. Here, Sanjek is at his best. He knew this material completely. No volume of information and memories about popular music is ever likely to eclipse Sanjek's volume 3. The many areas of silent-movie music, radio, records, stereo, Hollywood and television, and the related business affairs from licensing, copyrights, and arts management to payola, radio profanity, and racism are all covered with lucidity and insight. Although there are too many themes in Sanjek's volume to cover here, cultural and business historians should note his discussion of the way in which the cohesiveness of the music world invariably breaks down amid the increasing variety of aesthetic and entrepreneurial paths. Sanjek is no sentimentalist, but he understands the fragility of the artist's world and the sensitivity required of business people there if they are to be successful. Sanjek always exhibited that sensitivity. It is not something that can be summarily taught in business schools, nor, as Sanjek implies, can it be nurtured by large unions who regard artists as mere workers. As businessmen and artists have incessantly blamed each other for any malaise, the artists' world has grown ever narrower. If a healthier atmosphere is to be created, both sides must yield, and not just financially. How is that to be done? Reading Sanjek's volume 3 would be a good start.

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RUPERT WILKINSON. *The Pursuit of American Character*. New York: Harper and Row. 1988. Pp. viii, 166. \$16.95.

This brief essay by a British historian trained in the United States undertakes three projects: it surveys several efforts, between 1940 and 1980, to identify an American "national character"; it evaluates those efforts by mustering a series of counterexamples drawn

from three centuries of American history; and it offers the author's own formulation of what he considers distinctively American traits. As such, the book is both anomalous and ambitious. It defies the current consensus that "national character" studies obliterate crucial ethnic, racial, regional, class, and gender distinctions, and it risks generalizations at a time when the specialized monograph dominates historical scholarship.

The opening section describes, in a few paragraphs each, the contributions of such figures as Margaret Mead, David Riesman, David Potter, and Christopher Lasch to the "American character industry" (p. 7). Readers who look for insight into the biographical, intellectual, and social origins of those writers' works will be disappointed. Instead, Rupert Wilkinson concentrates on the verdict each text pronounces about the nature of individualism and conformity in American society. In general, he accuses his subjects of failing to chart multiple "character tendencies" in modern America. He finds recent interpretations of the decline of community particularly exaggerated, claiming that pervasive consumerism and privatization do not necessarily mean more self-absorption.

To remedy such alleged oversimplification, Wilkinson insists that the American experience has repeatedly "braided individualism with the call to affiliate" (p. 56). His presentation of that idea as his own discovery—the notion has been, after all, the organizing principle of countless American cultural history survey courses—is perhaps the most unfortunate aspect of the book. His seven specific instances of the tension between individualism and community (Puritanism in seventeenth-century Massachusetts, antebellum revivalism, and Progressivism, among them) are similarly unsurprising. More in the nature of an annotated list than an analysis, this part of the essay reveals Wilkinson's wide reading and synthetic skills. But his commendable integration of both old and new scholarship sometimes involves distortion of his sources, for example, when he cites John Mack Faragher's study of the overland journey to support his view that the wagon train supplied genuine community not only among men but also among women.

Wilkinson's attempt to sketch a national portrait is more imaginative. Americans, he suggests, have historically exhibited four fears: a fear of being owned, of falling apart, of winding down, and of falling away. Yet it is troubling that, although Wilkinson notes some socioeconomic developments that purportedly shaped each fear, he presents these anxieties as essentially timeless, deriving "from traditional ideas and values that have a life of their own" (p. 3). Thus, the fear of being owned, for example, remains the same whether or not the American scene includes slaveholders or modern corporations.

Nonetheless, the book does contain the valuable reminder that Americans have "often targeted criticisms onto their personal and spiritual qualities as a way of avoiding challenge to their basic institutions" (p. 53). Perhaps Wilkinson's survey may appeal to the

general audience for which it seems intended and spark some useful classroom debates.

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CHARLES LESLIE GLENN, JR. *The Myth of the Common School*. Amherst: University of Massachusetts Press. 1988. Pp. xi, 369. Cloth \$37.50, paper \$13.95.

Although Charles Leslie Glenn, Jr., deploys the code phrase "the myth of the common school," this book should not be construed as a polemic, although it does possess polemical force. The "common school" as symbol possesses a quick and effective potency for the professional educator and for a large section of our citizenry, middle-class and "liberal-progressive" in political orientation. The common school invokes fierce loyalty, representing not only an ideal of national unity but also an ideal of equality, comity, and social justice. In recent decades the ideal or idea of the common school, that is, the public school, has been called into question from many quarters. In the late 1960s and 1970s, a group of neo-Marxist social scientists reviled the public school as an instrument of class domination, while free school advocates and deschoolers condemned it as an institution that inhibited students' potential for creativity and self-fulfillment. The public school easily survived those challenges and maintained its essential hold on its middle-class constituency and the educational profession; the belief that the public school was vital to the country's well-being persisted. At present, however, the idea of the common school may be facing one of its greatest challenges.

The challenge arises from the school reform or excellence movement that has developed in response to the publication of the National Commission on Education's *A Nation at Risk* (1983) and is driven by the widespread perception that the country's public school system is in ruins. One of the reforms advocated by the excellence movement is parental choice, usually referred to simply as "choice," a plan that involves giving parents wide freedom and the means to send their children to schools of their choice, public or private, religious or secular. The means to enhance choice would be provided from allocation of public resources through some sort of education voucher or tax credit scheme. The usual argument on behalf of choice is that the private school sector offers an education superior to that of the public school system and that choice would lead to a radical improvement in public education by forcing the public school system to compete with the private sector for students. Despite the growing appeal of this argument, it is an option that the educational profession traditionally has resisted and would continue to resist adamantly should legislation to foster choice at the state or national level be proposed. In its opposition to choice, the educational profession has in the past been joined by the public school's

middle-class constituency. Although choice is not yet a practical policy option, one can predict that there will be a heated public debate over the issue in the next few years. Glenn's book may best be understood in the context of this discourse and this debate. Its intent is to influence public policy toward choice through a reorientation of the public perception of the idea of the public school.

Glenn examines the "myth of the common school" from two angles: first, the history of the idea of the common school as it emerged in the thinking of Horace Mann and from the common school struggle in nineteenth-century Massachusetts; and, second, the history of the nineteenth-century common school struggles in France and the Netherlands. Glenn's study of Mann's thought and his conclusion that the ideal of the common school was far from ever being realized in practice in Massachusetts or anywhere else in the United States will, with one important exception, not contain much that is new to those familiar with American educational history. Glenn praises Mann and his fellow school reformers for their generous vision of universal education but faults them for their lack of generosity toward the rights of parents: their lack of respect for the "stubborn particularities of loyalty and conviction . . . and world views . . . by which parents live and by which they hope their children will also live" (p. xi).

Glenn's major contribution is in the comparative history of education. He juxtaposes the common school struggle in the United States during the nineteenth century with the school struggles being waged at the same time in France and the Netherlands. Glenn's point is that in all three countries the real issue at stake involved not so much the quality of education or national cohesion as parental rights and freedom of conscience. In France and the Netherlands and, in fact, in all Western democracies save ours, Glenn observes, the decision was made to provide some mix of control between parents and state as to what kind of school a child attended and what he or she studied. As a practical matter, that meant state financial support for private schools, religious as well as secular, without the dire results predicted or anticipated by partisans of the common school.

Although Glenn is an advocate of parental choice, he is scrupulously fair in discussing the problems inherent in this policy. He points out that the argument over choice has, because of the recent influx of immigrants, been revived in France and the Netherlands, and he remarks on the potential of choice for encouraging divisive ethnocentric and exclusivist tendencies. But he comes down finally on the side of parental rights and freedom of conscience. The question Glenn ultimately raises is whether public educational policy in the United States should be radically altered to foster parental choice. Glenn concludes that, if the responsibility and the means for educational choice are not given or returned to parents, the American public school will in the end neither achieve excellence nor retain the support of its middle-class constituency.

This book is a thoughtful and balanced work. It not only helps provide a corrective to the parochialism typical of histories of American education but also makes a significant contribution to the current debate about education in America, which is conducted with a seeming lack of any awareness of relevant educational debates in Western Europe, England, or Canada for that matter.

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ANNE M. BOYLAN. *Sunday School: The Formation of an American Institution, 1790–1880*. New Haven: Yale University Press. 1988. Pp. xii, 225. \$26.50.

Anne M. Boylan's book is a valuable addition to the growing body of scholarship on American social and cultural history. As a result of her efforts, the Sunday school takes its place among the institutions that helped organize and define the rapidly changing world of the nineteenth century. In crisp and polished prose, Boylan argues that the Sunday school played a major role in uniting and disseminating the ideologies of republicanism and evangelicalism, imparting to children "the values of self-control, obedience, good habits, and fixed principles" necessary for survival and success in an urbanizing, industrializing nation (pp. 164–65).

Boylan's work is firmly rooted in the new social history. She deftly weaves into her story insights derived from recent work on the changing roles of women, attitudes about childhood, challenges to orthodox Calvinism, and the development of middle-class values. She correctly points out that the motivations of the founders of Sunday schools were intricate, at the same time disinterested and class-defined. Students, too, responded in a variety of ways to the new institution—some dutifully mirroring the ultra-pious conduct of fictional role models and others merely attending long enough to secure coveted prizes. By focusing narrowly on a single institution, Boylan is thus able to capture for the reader much of the complexity of everyday life often lost in more sweeping studies.

One of the most interesting elements of Boylan's work is the light her study sheds on national debates over states' rights and slavery. As in so many other areas, Americans involved in the antebellum Sunday school movement were unable to prevent these disagreements from fragmenting their lives. Early millennial hopes faded, and the power of national organizations, such as the American Sunday School Union, dwindled in the face of local opposition.

After 1865, however, pragmatism triumphed over dogma as teachers from many denominations and regions participated in local, state, and national conventions to share ideas and learn modern techniques. Boylan observes that this drive toward professionalization "symbolized the emerging tendency towards consolidation in American culture" (p. 90). Thus, through-

out the period under discussion, the Sunday school reflected key ideological and social developments.

In a few ways, however, the book fails to satisfy. Boylan set out "to illuminate the entire process of institution building and to connect it to the economic and social changes that transformed the United States in the nineteenth century" (p. 1). It seems odd, therefore, that so fundamental an issue as immigration did not shape Boylan's argument. And, if fear of Roman Catholicism was as widespread as Ray Billington and others would have us believe, what impact did nativism have on the Sunday school movement?

On the whole, though, this is a useful study. In spite of its narrow scope, it illuminates a wide variety of important trends, especially the establishment of new and specialized institutions for the indoctrination of the young and the efforts of middle-class Americans (particularly women) to create a nationwide evangelical "mental universe" (p. 70).

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CHARLES H. LIPPY and PETER W. WILLIAMS, editors. *Encyclopedia of the American Religious Experience: Studies of Traditions and Movements*. In three volumes. New York: Charles Scribner's Sons. 1988. Pp. xvi, 665; x, 669-1265; x, 1269-1872. \$225.00 the set.

This work, edited by Charles H. Lippy and Peter W. Williams, represents a publishing event of grand proportions to students of American religious history and to general readers. Set in a massive three-volume format, it traces both the effect of particular religious movements on American history and the influence of the American environment on particular religions. The topics span such diverse areas as literature, philosophy, music, architecture, film, and government. Included in the 106 topics are many original essays by leading scholars that move beyond descriptive summaries to establish original insights and novel conclusions.

Besides bringing recent scholarship together in one multivolume series, the encyclopedia is new in several key respects. First, the term "American" is inclusive and meant to imply more than the United States. Significant essays on American Indians, Mexico, Canada, and Spanish Catholicism all contribute to a broadened understanding of the New World. Although the bulk of attention is still clearly oriented to the United States, the editors are to be praised for avoiding the narrow provincialism of earlier religious surveys limited to North America.

Also novel in this series is the focus on religious experience rather than straight denominational history. "Experience" is the key word here. In an opening discourse on method and approaches, the editors make plain that their subject matter is defined less by the particular histories of churches and doctrines than by general cultural experiences of the sacred that may or may not be confined to ecclesiastical settings. Thus,

alongside treatments of conventional subjects such as Puritanism, Methodism, Judaism, and Roman Catholicism, the reader will discover serious considerations of such diverse themes as civil religion, the novel, mass communications, and the occult. Popular culture clearly looms large in the editors' minds as they move beyond the religious dimensions of "high culture" to examine newspapers, comics, *Star Wars*, and *The Empire Strikes Back*.

To an admirable extent, the editors have avoided the myopia characteristic of past studies in which all of American religious experience was viewed through the lenses of "mainstream" Protestantism and Reform Judaism. Within Christianity, the reader discovers considerable attention to previously ignored (or reviled) groups such as Christian Scientists, Adventists, Mormons, fundamentalists, Pentecostals, and charismatics (both black and white). Among essays on Judaism, the reader encounters comprehensive discussions of Conservative and Orthodox Judaism, Jewish literature, and Judaism in contemporary America. Finally, and most usefully, there are generally clear and original essays on important, but surprisingly understudied, topics such as Buddhism, Hinduism, Islam, free thought, and black separatist movements.

The topical approach taken by the editors is eminently sensible given the expanse of the terrain covered by the essays. In general the essays are grouped in eight categories that move from "insider" histories of particular churches and movements to "outsider" perspectives that examine American religion within the larger context of politics, education, and the arts. Throughout, the reader is treated to a literary seminar that weaves the threads of American religion and American history into mutually reinforcing patterns of interaction, which demonstrate how impossible it is to understand American society without understanding religion.

In selecting contributing authors for the volumes, the editors made two wise decisions. First, they selected the finest scholars in the field whose work represents the cutting edge of contemporary scholarship. The roster of contributors reads like a Who's Who of American religious, literary, and intellectual history. With such a slate of contributors, the editors made a second wise decision in giving these authors the extended space that enabled them to take their assignments seriously. Rather than adopt an abbreviated dictionary format, the editors opted for depth and gave the writers free rein in developing their topics. For the most part, the authors do not disappoint. The occasional weak essay is more than offset by outstanding essays that average twelve pages in length with full bibliographies. In many cases the authors move beyond traditional treatments (especially of twentieth-century topics) to advance the state of scholarship in their fields. Of particular note here are essays dealing with black religion, Pentecostalism, and women.

Readers will undoubtedly discover topics and movements not included in this set. But, whatever the

deficiencies, this encyclopedia is the finest to date and will undoubtedly become the standard reference tool for the study of American religion in the generation to come.

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CHRISTOPHER J. KAUFFMAN. *Tradition and Transformation in Catholic Culture: The Priests of Saint Sulpice in the United States from 1791 to the Present*. New York: Macmillan. 1988. Pp. xviii, 366.

With the publication of this volume, Christopher J. Kauffman has confirmed for himself a place among the elite in American Catholic scholarship. Kauffman was already highly regarded as editor of the journal *U.S. Catholic Historian*, applauded for his role as general editor of the six-volume bicentennial history of Roman Catholicism in the United States, *Makers of the Catholic Community* (1988), and respected for his earlier tome on the Knights of Columbus, *Faith and Fraternalism* (1982). But with this book he has produced his finest work to date.

Although the depth of Kauffman's research clearly establishes his study as a major contribution to American Catholic historiography, this book is in scope more than that. Divided into a prologue and three parts, it is one of the best treatments yet produced by an American Catholic historian on the subject of Catholic spiritual formation as it developed in the seminary training of one particular religious or secular congregation. In his prologue Kauffman focuses exclusively on the Sulpician seminaries of France, but he does inject into his narrative the Sulpician association, on occasions, with other congregations such as the Vincentians and the Eudists. The primary calling of the Sulpicians, as their founder, Jean-Jacques Olier defined it, was to mold candidates for the priesthood. Thus the Sulpician seminaries were usually national ones. But many young men who received the Sulpician training went on to serve in other congregations or as seculars, leaving the influence of the Sulpicians, from their establishment in 1641 on, much broader than their own relatively small numbers might have warranted. It is in presenting and analyzing these developments that Kauffman is at his best.

Having traced the French spiritual, academic, and ecclesiastical character of the Sulpicians in his prologue, Kauffman then treats the American Sulpician experience in the first three parts of his study. Complementing Annabelle M. Melville's fine two-volume work (1986) on the Sulpician Bishop Louis William DuBourg, Kauffman carefully details the Sulpicians' establishment in 1791 at Baltimore, initiating his coverage of the congregation in the United States. That aspect of his book, by far the largest portion, is divided into three sections: "The Sulpicians and the Formation of the American Church (1789–1850)," "Americanization in the Immigrant Church (1850–1911)," and

"From Romanization to the Second Vatican Council (1911–67)." He fashions a scholarly analysis of the early to mid-nineteenth-century interaction between the French and American members of the congregation, highlighting the cultural and national tensions that grew between them as well as the conservative and liberal tendencies of different factions. At the same time, however, Kauffman scrupulously brings out the likenesses that all of the Sulpicians shared as priests who were common heirs of the *esprit ecclésiastique* of Olier and Jacques-André Emery, the greatly admired superior general of the Sulpicians from 1782 to 1808.

In the second part of his book, Kauffman, with great care, structures the expansion of the Sulpicians from New York to San Francisco during the middle and latter decades of the immigrant period in American history. And he confronts directly the Americanization crisis that engulfed the church in the United States during those years. The final section of Kauffman's study carries the account of the Sulpicians beyond the Second Vatican Council and thoroughly assesses the metamorphosis of the congregation in the post-Vatican II era.

This study does contain a relatively minor factual error, but one, nonetheless, of which the reader should be made aware. In referring to Archbishop Anthony Blanc of New Orleans on page 136, Kauffman appends the letters, "C.M." to the prelate's last name. That would indicate that the archbishop was a Vincentian, but he was a secular. This insignificant mistake takes nothing away from the fact that Kauffman's book is a thoroughly researched, wonderfully written piece of scholarship that will remain a classic study in the field of spiritual and institutional histories of secular congregations and religious orders, male as well as female, for years to come.

PATRICK FOLEY

Journal of Texas Catholic History and Culture

RICHARD T. HUGHES and C. LEONARD ALLEN. *Illusions of Innocence: Protestant Primitivism in America, 1630–1875*. Foreword by ROBERT N. BELLAH. Chicago: University of Chicago Press. 1988. Pp. xviii, 296. \$29.95.

This book sets forth a bold and provocative thesis. Richard T. Hughes and C. Leonard Allen argue that the "restoration" idea has been a central feature of American culture from the seventeenth century to the present day. Time and again, American leaders (both religious and secular) have attempted to escape the confines of history and return to an idealized, primordial past. The entire book is a demonstration of this thesis.

Drawing on the work of Theodore Dwight Bozeman, the authors begin with a discussion of John Cotton and Roger Williams. They view the Puritan experience through the lens of "a restorationist crusade" (p. 31). This idea is interesting enough, but when they reach the sections on the antebellum world, Hughes and Allen break into full stride. Their analysis of the primitivist

dimension of the early Baptist tradition, the Latter-day Saints, and the Disciples of Christ is most convincing. Indeed, it would be hard to argue otherwise.

Their case weakens slightly with the chapter on New Orleans Presbyterian cleric Benjamin M. Palmer and his "Civic Theology for the South." It collapses into confusion and lunch-table conversation with their discussion of the American Myth and Allan Bloom's recent book, *The Closing of the American Mind* (1987).

The work contains a slight hidden agenda. The authors suggest that the continuing search for "historyless" universals lies at the root of American national arrogance. They find this especially marked in the nineteenth-century march across the continent and in the (continuing) realm of foreign policy. Because Americans believed that they had tapped into primitive, "universal" truths, they were not loath to force those truths onto other nations. The authors maintain that, although the search for innocence began in a cosmic, open fashion, it soon collapsed into cultural narrowness. Thus, Hughes and Allen argue, "Our concern . . . is that Americans learn fully to admit and appreciate the finite historic context in which they inevitably live and move and have their being" (p. xvi). Consequently, this book is nine parts historical analysis and one part sermon. It is a unique blend.

One simply has to accept the book on its own terms. If one is willing to do this, however, it has a great deal to say. The authors' analysis of primitivism is the most sophisticated we have seen to date. Prior to this, American cultural ideas have usually been described in terms of abundance, individualism, and millennialism. But thanks to Hughes and Allen, "restorationism" now has to be included as well.

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SANG HYUN LEE. *The Philosophical Theology of Jonathan Edwards*. Princeton: Princeton University Press. 1988. Pp. xiii, 248. \$35.00.

In his analysis of Jonathan Edwards's philosophical theology, Sang Hyun Lee argues that dispositional ontology correctly identifies Edwards's contribution to a restatement of Augustinian-Calvinist tradition in theology. According to Lee, Edwards shifted away from the use of form and substance, as traditional Western metaphysical conceptions, to originate a conception of God and the world as active and abiding principles that possess reality separate from their manifestation in actions and events.

Lee portrays Edwards's philosophical theology as a coherent framework by which "a truly modern conception of the universe" is presented (p. 12). For scholars of Edwards, Lee's thesis marks an advance over the earlier work by Douglas J. Elwood (*The Philosophical Theology of Jonathan Edwards* [1960]), who claimed that Edwards posited a God who is perfectly complete and yet created the world based on the idea of creativity as

self-communication, that is, "an activity of giving rather than receiving, and thus an activity of an already complete and perfect being" (see Lee, p. 171). But Lee, while crediting the correctness of this view as far as it goes, extends Edwards's treatment of this problem by claiming that this God is also expanded by essentially an active and rational power (chap. 7: "The Increasing Fullness of the Divine Being"). God is extending or repeating his inner glory in and through the regenerate human mind's knowledge and love of that divine creativity.

Most interestingly, in his concluding chapter, "God and the Becoming World," Lee applies Edwards's dispositional ontology to postmillennialism. Edwards's postmillennialism reflected his metaphysical understanding that God not only is in history, accomplishing the work of redemption, but also will transform all of creation. Although the time of peace and prosperity mentioned in Rev. 20 is depicted by Edwards "as the true realization on earth of God's end in creation," it is also a "never-ending process" (p. 216). This end is achieved for Edwards because there is a dynamic rhythm to reality not only between world, believer, and God but also between church, Christ, and God, which continues into the future because Edwards believed that God's very nature was dynamic, or processional. In Edwards's own words, it is the end that "will never have an end" (p. 237).

I have at least two questions concerning Edwards's philosophical theology. Is the increase of being from God inevitable in the individual, or can human will block divine being? Second, why could Christ not come before the millennium (premillennialism) and still accomplish God's endless repetition?

Lee's book establishes him at the pinnacle of scholarship on Edwards. He significantly advances the dynamic element in Edwards's theology that has eluded so many others. His book also continues a historical affirmation that Edwards's thought was, and is, vital to the philosophical and theological enterprises.

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NEAL L. TOLCHIN. *Mourning, Gender, and Creativity in the Art of Herman Melville*. New Haven: Yale University Press. 1988. Pp. xviii, 234. \$23.50.

On January 28, 1832, after several weeks of intermittent mania, Alan Melvill died, leaving behind in poverty a patrician wife and eight children, including his twelve-year-old son Herman. Neal L. Tolchin argues lucidly and persuasively that Herman Melville's unresolved grief over his father's death played a major part in shaping his fiction. Maria Melville, in her ambivalent rage over her husband's death, deposited in her son's consciousness an image of his father as a "damned deathbed maniac" (p. 118), which rendered him incapable throughout his life of completing his own mourning. That situation was reinforced by Herman Melville's essential acceptance of Victorian cultural codes

that assigned to women the public symbolization of bereavement and placed severe demands for emotional self-control on all mourners. Under those psychic and cultural conditions, Tolchin argues, Melville's writings explored his own buried grief and rage in a variety of ways, ranging from the anthropological observations on comparative mourning customs of *Typee* to the disturbingly parodic treatment of the unresolved mourning of the protagonist in *Pierre; or, The Ambiguities*.

In this study Tolchin convincingly demonstrates that Melville's work abounds with images of blocked bereavement and with thinly veiled references to the death of his father. In steadily tracing those concerns throughout the major novels (without ever reducing them to mere "grief work"), Tolchin offers a compelling synthesis of the body of Melville's work and manages to solve a number of literary puzzles along the way. A major strength of this study is Tolchin's success in moving beyond a purely Freudian approach to draw on recent scholarship in the psychology, sociology, and anthropology of mourning and to apply those twentieth-century findings to an understanding of Melville only after due attention to nineteenth-century cultural context. Tolchin cites, for example, the findings of the Harvard Study of Bereavement that, whereas women experience bereavement in terms of abandonment, men view it in terms of dismemberment. But he applies those findings to Melville's work (most successfully, not surprisingly, to *Moby Dick*) only after he locates a similar pattern in nineteenth-century mourning manuals and explains it historically with reference to the ideology of separate spheres.

The overall effectiveness of this study falters in its conclusion, which argues that, when the buried conflicts of Melville's grief break through the surface of his fiction, his work radically critiques prevailing social codes. Tolchin raises this issue of Melville's radicalism too late in his study to make the case. Having thoroughly demonstrated that Melville's mourning for his father remained unresolved, he is less persuasive in suggesting that the novelist ever managed to escape "the double bind in Victorian America's desire both to prolong mourning and to control and limit the expression of grief's conflicts" (pp. 162–63). But in a sense that is to say Tolchin is a victim of his own success. He has produced a deeply insightful study not only of Herman Melville but also of the psychic impact of the "double bind" of Victorian sentimentalism, which should prove useful to historians of nineteenth-century American culture.

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MARGO J. ANDERSON. *The American Census: A Social History*. New Haven: Yale University Press. 1988. Pp. xiii, 257. \$32.50.

Since 1964, when Stephan Thernstrom's *Poverty and Progress* popularized the research potential of the manuscript censuses, historians have pored over the

lists. The bible of the trade is Carroll Wright and William Hunt's *History and Growth of the U.S. Census* (1900), which described and reprinted the original questionnaires. Margo J. Anderson's book augments rather than supplants Wright and Hunt by detailing the workings of the Bureau of the Census since 1790 in the context of the sociopolitical controversies engendered by the decennial enumerations and statistical reports. Although subtitled "social history," the book's central theme is the "politics of counting" (p. 213). If information is power, no government agency has controlled the source and flow of data more than the Census Bureau and its statistical handlers. The political fallout of the census reports has embroiled the agency in the complex issues of congressional reapportionment, sectionalism and slavery, black disfranchisement during Reconstruction, racism and immigration restriction during the era of World War I, unemployment during the Great Depression, federal revenue sharing, unregistered aliens, and social engineering during the 1960s and 1970s. The census reports thus served as lightning rods for the nation's political controversies, and Anderson puts that theme front and center.

As the "first new nation," the United States began the policy of regular population counting in order to redistrict political and economic power among the states "according to their respective Numbers" (p. 9). Such periodic undertakings in a burgeoning nation necessarily made the "figure factory" (p. 116) itself a pioneering institution in the creation of modern social science and information-processing techniques. The book's secondary theme is the introduction of high-tech methods from simple cross-tabulations and Hollerith punch cards to UNIVAC vacuum-tube computers, optical scanners (FOSDIC processing), and supercomputers.

Since the heart of the census is the questionnaire, Anderson traces the development of census categories from the simple household count of 1790 to the detailed "social indicators" of 1970, which even enumerated bathroom facilities. She particularly stresses the occupational and other economic questions that were long demanded by the major outside advocacy group, the Census Advisory Committee, which was chosen by the American Statistical Association and the American Economic Association. But Anderson ignores one of the most important social behavioral variables in American history, namely, religion. Did the census keepers ever consider adding a religious question when they substantially revamped the schedules in 1850, 1880, 1900, 1950, 1970, or 1980 (when the ancestry question was added)? If not, why not? The author even overlooks the one instance—the Current Population Survey of March 1957—when the Census Bureau did in fact ask a question on religion.

Anderson has mastered all the pertinent primary sources and secondary works and has mined the files of the Census Bureau Library, the National Archives, presidential libraries, the *Congressional Record*, the *New*

York Times, and federal court decisions. Her book is the first comprehensive chronological account from 1790 through 1980 of what the Census Bureau did and does and why it is important. It is must reading for political, social, and institutional historians. The author's lucid style makes the task a pleasure.

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JOHN BRAEMAN. *Before the Civil Rights Revolution: The Old Court and Individual Rights*. (Contributions in Legal Studies, number 41.) New York: Greenwood. 1988. Pp. ix, 216. \$37.95.

This book is a failed attempt at revisionism. According to John Braeman, "the Old Court's record on civil rights/civil liberties issues was far from simply one of judicial negativism. On the contrary, much of what the Modern Court has done when viewed in long term perspective appears incremental expansions upon precedents laid down by the Old Court" (p. viii). That thesis is novel and provocative, but Braeman comes nowhere near proving it.

Indeed, he fails even to define the "Old Court." Throughout much of the book, that term seems to refer to the Supreme Court between Reconstruction and 1937. In chapter 5 Braeman discusses enough pre-Civil War cases, however, to raise serious doubts about what he means by the term.

Besides failing to delineate his topic carefully, Braeman also makes no real effort to build a case for his thesis. In addition to an introduction and a conclusion, the book has chapters in which he examines how the Old Court dealt with constitutionally protected rights, race, and criminal procedure. Those substantive chapters are just discussions of cases, organized with no reference whatever to Braeman's argument. None has so much as an introductory or concluding paragraph relating its contents to his thesis.

Much of the evidence Braeman presents seems inconsistent with that thesis. In addition, little of it is new. Students of constitutional history will find most of what Braeman has to say familiar. He offers a very useful summary of changes in Supreme Court jurisdiction and a few new insights into the constitutional aspects of criminal procedure during the pre-incorporation era. But on subjects such as civil rights, freedom of religion, freedom of expression, gender discrimination, voting rights, and labor law, he offers nothing novel.

One reason for the lack of new evidence is inadequate research. Braeman's footnotes, which constitute nearly 40 percent of the book, evince an exhaustive investigation of the relevant secondary works. Unfortunately, his primary sources are limited to judicial opinions and old law review articles. They include neither the Supreme Court records and briefs nor any manuscript material. Rather than seeking reasons for decisions in the readily available papers of justices who served on the Old Court, Braeman relies on the writ-

ings of others, and even on outright speculation, to explain judicial motivation.

Both his research and his organization are reminiscent of a law review article. The chapter entitled "The Criminal Defendant," for example, is excellent but utterly ahistorical. Braeman discusses legal categories one after another, using cases drawn from a period of more than a century. The result is interesting doctrinal analysis, but it is not history.

Historian Braeman should leave that sort of thing to lawyers and concentrate on what he is best qualified to do: placing judicial decisions in proper social and political context. Instead, he drifts into doing law office history and winds up committing the sort of blunder associated with that genre. "*Norris v. Alabama* (1935) marked the passing of the all-white jury as a bulwark of the South's caste system," Braeman assures his readers (p. 85). Perhaps that is what appellate opinions suggest, but anyone familiar with the way all-white juries protected from punishment Ku Klux Klan members who sought to intimidate assertive blacks and white civil rights workers during the 1950s and 1960s (as Braeman surely is) knows better.

Such errors are not typical of this work. It is a competent and well-written little book. Unfortunately, it is really part of a larger study. As Braeman explains, this book is an outgrowth of a project on the civil rights revolution itself. It appears to be a background chapter that got out of hand. Mere length does not justify separate publication. Research that is adequate for a background chapter is insufficient for a monograph that purports to make an original contribution. Nor does imposing a revisionist thesis on a manuscript that seems to have been written without any reference to it turn that manuscript into a book. *Before the Civil Rights Revolution* is really a prolonged and promising prologue. I hope that Braeman will soon publish the rest of the book.

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DEBORAH J. BARROW and THOMAS G. WALKER. *A Court Divided: The Fifth Circuit Court of Appeals and the Politics of Judicial Reform*. New Haven: Yale University Press. 1988. Pp. xiv, 274. \$29.95.

The influence of political conflict on the reform of the federal court system is an important but little-examined subject. In 1980 President Jimmy Carter signed legislation splitting in two the U.S. Fifth Circuit Court of Appeals and creating the Eleventh Circuit Court. Carter's action received attention primarily from legal specialists, federal jurists, and lawyers. Yet the comparatively unnoticed event was the culmination of a twenty-year campaign that was often central to the nation's civil rights struggle. In this fine book, Deborah J. Barrow and Thomas G. Walker explore the relation of that campaign to civil rights issues and provide deep insight into the dynamics of federal judicial reform.

Article III of the Constitution left the establishment and ultimate control of the federal judiciary to Congress. Barrow and Walker graphically demonstrate, however, the degree to which Congress generally leaves administration of the judicial system to federal judges themselves until a crisis develops involving some controversial national issue. During the 1950s and 1960s, the Fifth Circuit Court of Appeals was caught between the need for fundamental administrative reform and the civil rights struggle. The Fifth Circuit encompassed the Deep South states of Texas, Louisiana, Mississippi, Alabama, Georgia, and Florida. That region was experiencing simultaneously dramatic growth associated with the emergence of the Sunbelt and tragic racial clashes following the Supreme Court's decision of *Brown v. Board of Education* (1954). In 1950 the Fifth Circuit had seven judges and received 408 appellate cases annually. By 1960 the court's docket had risen 41 percent to 577 cases, and within another three years the number had increased 51 percent, making it the heaviest federal appellate work load per judge in the nation. From the early 1960s on, judges, experts, and elected officials debated whether the best remedy for the ever-growing caseload was a division of the Fifth Circuit in two, with one court retaining the name of the old Fifth and the other new appellate tribunal designated as the Eleventh Circuit Court.

Throughout the early 1960s, the number of judges remained the same. Most of the cases the court considered involved economic or procedural issues. But what attracted national and international attention was the Fifth Circuit's adjudication of suits arising from the white South's massive resistance to the civil rights movement's nonviolent crusade for equal justice. The Supreme Court upheld most of the Fifth Circuit's decisions arising from confrontations in Mississippi, Alabama, and the other Deep South states. Those opinions, along with the leadership of Martin Luther King, Jr., encouraged Congress to enact the revolutionary Civil Rights Act of 1964 and Voting Rights Act of 1965.

A few Fifth Circuit judges, such as Richard Rives of Alabama and Minor Wisdom of Louisiana, feared that the breakup of their court would weaken its ability to continue the effective defense of civil rights. At the same time, zealous segregationists, such as U.S. Senator James O. Eastland of Mississippi, supported the circuit's division primarily to increase the influence of the minority of Fifth Circuit judges who declined to enforce equal rights vigorously.

Until the mid-1970s the clash between the positions represented by Eastland and by Rives and Wisdom thwarted judicial reform. By that time the Fifth Circuit was handling civil rights issues as well as, if not better than, the rest of the nation's federal appellate courts. Meanwhile, the continuously expanding caseload had become an even more pronounced problem. Yet factional political pressures involving civil rights concerns delayed congressional action until after Eastland re-

tired, paving the way for the reform measure Carter signed in 1980.

Using a wide range of primary sources, including interviews and private papers, Barrow and Walker tell this story thoroughly and well. They have achieved the enviable result of writing a case study that can serve as an introduction to an entire field of significant research.

TONY FREYER
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DIAN OLSON BELANGER. *Managing American Wildlife: A History of the International Association of Fish and Wildlife Agencies*. Amherst: University of Massachusetts Press. 1988. Pp. xv, 247. Cloth \$25.00, paper \$11.95.

This book is the authorized biography of a somewhat unusual special interest group. Although its membership includes five federal agencies, Mexican and Canadian wildlife services, and eight Canadian provinces, the International Association of Fish and Wildlife Agencies (IAFWA) represents primarily the needs and desires of U.S. state wildlife management agencies.

Dian Olson Belanger traces the organizational history of the IAFWA from a meeting in 1902 of state conservation leaders held in Yellowstone National Park to the Sage Brush Rebellion and lead-shot controversies of the past decade. In the course of the chronicle, she provides a lucid and useful review of the history of wildlife conservation in the United States as seen from the association's perspective. I found it interesting reading.

The IAFWA was a product of America's first great wave of conservation sentiment, a wave produced by the threatened extinction of forests and wildlife in a period best known for conservation luminaries Theodore Roosevelt and Gifford Pinchot. Like Pinchot and Roosevelt, the IAFWA argued for professional game management as a part of scientific conservation. Unlike Pinchot and Roosevelt, members of the IAFWA, being employees of the several states, were committed to protecting state sovereignty with respect to management of resident fish and game.

Indeed, much of the narrative is devoted to turf battles where the good guys of the IAFWA locked horns with others. Local interests are described as "parochial"—an adjective many might apply to the IAFWA itself—and the institutions of a wider world are viewed with distrust. The federal government is described as "encroaching" and international agreements that threaten state jurisdiction as "too hastily concluded" (p. xiii).

Belanger does not misrepresent or belittle views that differ from those of the IAFWA, but she assumes, rather than arguing for, the legitimacy of the IAFWA's commitment to a states' rights perspective on game management. Following American ratification in the mid-1970s of the Convention on International Trade

in Endangered Species (CITES), she reports, "High hopes soon turned to dismay for the American state game conservationists as the CITES agreement, like other protective laws before it, threatened their authority over *their own* wildlife [emphasis added]" (p. 134).

As a historian, Belanger suffers from a relative lack of information concerning the association's early years. Most of what is available comes from the somewhat sketchy proceedings of annual conventions. As a result, in the early chapters of the book, she relies on traditional secondary works, such as James B. Trefethen's *Crusade for Wildlife* (1961), surmises a good deal where the written record is scanty, and descends occasionally into minutiae, with which, I suspect, the proceedings are abundant. Who but an organizational aficionado would care to know that the IAFWA was fifty years old before it took the prudent step of investing its cash surplus in an interest-bearing savings account?

In recent decades the nation has focused greater attention on environmental issues, the IAFWA has been a more significant player in Washington politics, and there has been no shortage of materials on which the historian might draw. In the corresponding chapters, one reads with increasing interest as the conservation values of the IAFWA are challenged not by the exterminators and exploiters of its early history but by protectionists, animal rights groups, federal agencies, and international organizations, each committed to a different, and sometimes a radically different, conservation ethic. These chapters expose the important and frequently unresolved issues of American conservation policy, and one readily understands how positions taken by the IAFWA follow naturally from the association's history, interest, and constituency.

This study is an effective introduction to wildlife politics, to the work of the IAFWA, and to the implications of federalism and interest group politics for environmental management in the United States. Scholars will appreciate the forty pages of appendixes, the complete documentation, and the carefully prepared index.

CRAIG W. ALLIN
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ROBERT LAWSON-PEEBLES. *Landscape and Written Expression in Revolutionary America: The World Turned Upside Down*. (Cambridge Studies in American Literature and Culture.) New York: Cambridge University Press. 1988. Pp. xii, 384. \$39.50.

Here indeed, as the author claims, is a new reading of works by revolutionary and postrevolutionary figures—not only belles-lettres by J. Hector St. John de Crèvecoeur, Philip Freneau, Hugh Henry Brackenridge, Timothy Dwight, and Charles Brockden Brown but also scientific efforts by Jedidiah Morse, Benjamin Rush, Noah Webster, and Thomas Jefferson. While Robert Lawson-Peebles applies close textual analysis to these works, he adopts the viewpoint of a perceptual

geographer concerned with the way people have tried to order their environment through language. Furthermore, he provides extensive historical background from the Old World. Readers may already know that Crèvecoeur dedicated *Letters from an American Farmer* to the Abbé Raynal, that Brockden Brown's villains reflect the Illuminati scare, that Jefferson took pains to refute the theories of Count Buffon, and that at Yorktown the British surrendered to the tune of "The World Turned Upside Down," but Lawson-Peebles puts new meaning into these old facts.

Three themes or motifs dominate this book. First, our early authors saw their environment as frighteningly unstable. Picturesque and even beautiful at times—a cisatlantic garden—it could turn into a gothic wilderness of disease, confusion, and crouching animals. Brockden Brown in *Edgar Huntly* achieves the most memorable scenes of this terrestrial mutability. Second, knowing through captivity narratives that not a few settlers chose to remain outside civilization, our writers feared a general regression among America's inhabitants. The country that Tom Paine and others had termed an asylum for the oppressed might become instead a nightmarish landscape of humans transformed into cannibals and beasts. Brackenridge offered several such scenes, and those who objected to the Louisiana Purchase offered others, but, of course, Brockden Brown and Edgar Allan Poe surpassed them all. Third is the motif that Lawson-Peebles calls the "instable relation between the text and the terrain" (p. 263). Most writers bemoaned their inability to describe the landscape and their response to it. Some tried images of amplitude or employed negation. In their search for order, they fell into the error of geographical symmetry, claiming that the Rocky Mountains were identical to the Appalachians or that the Mississippi must have an east-west counterpart, the Northwest Passage. Although Jefferson sent Meriwether Lewis to college in preparation for his excursion through the Louisiana Purchase, Lewis's style (whether literary or scientific) failed him once he reached *terra incognita*.

Lawson-Peebles takes several impressive sweeps through American literature to prove that his themes survive in modern times. He even provides an epilogue on Poe's fiction, in particular *The Narrative of Arthur Gordon Pym*. One wishes that he had also included Washington Irving, who developed (as Wayne Kime has shown) not only an American attitude toward the terrain but also the language with which to express it.

The book is well indexed and has an ample bibliography. Those who admire John R. Stilgoe's *Common Landscape of America, 1580 to 1845* (1982) should find Lawson-Peebles's work intriguing as well.

JAMES T. CALLOW
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EDWIN WOLF II. *The Book Culture of a Colonial American City: Philadelphia Books, Bookmen, and Booksellers*. (Lyell

Lectures in Bibliography, 1985–86.) New York: Clarendon Press of Oxford University Press. 1988. Pp. viii, 227. \$35.00.

Was there a literary canon, an acknowledged set of Great Books, in the eighteenth-century American colonies? This slender volume provides an extensive foundation for an answer to that question. Edwin Wolf II attempts to name all of the titles of books known to be in the possession of Philadelphians in that century. His evidence comes from booksellers' and library catalogs, newspaper advertisements, shipping invoices, purchases by subscription libraries and individuals, and inventories of estates and wills. Wolf takes pains to state clearly that his work is primarily a contribution to bibliographic history and not at all a study of the social history of the book. But there is abundant material in his taxonomic inventory of what late colonial Philadelphians read that will surely be grist for the mill of the growing numbers of scholars who practice *l'histoire du livre*.

Wolf groups his bibliography into five main sections: Bibles and religious texts, schoolbooks and practical manuals, histories and political texts, law books, and novels. He wisely declines to work up a statistical analysis of the quantities of each title, because his evidence comes from scattered and nonrandom sources. But he does offer his impressions of which books were in most demand or were most likely to be found on the bookshelves of well-known Philadelphians. Not surprisingly, religious books—Bibles, psalters, prayer books—headed the pack. More surprisingly, books on navigation and seacraft outnumbered titles of popular literature. Tacitus and Livy were widely owned, and, after 1760, novels such as *Pamela*, *Clarissa*, and *Fanny; or, The Amours of a West-Country Lady* were favorites in book catalogs and library loan records.

There is a very brief discussion of chapbooks, those small pamphlets printed cheaply and aimed at a popular audience whose tastes ran more to tales such as *The Prodigal Daughter* and *Reynard the Fox* than to Plutarch's *Lives*. But because chapbooks had a low rate of survival, compared to books, and because they were so inexpensive, they rarely left tracks in the kinds of sources Wolf uses. Also seriously underrepresented in Wolf's bibliography is the large pamphlet literature that Bernard Bailyn has argued, in his book *The Ideological Origins of the American Revolution* (1967), was central to the political education of the revolutionary generation. John Trenchard and Thomas Gordon's *Cato's Letters* and *The Independent Whig* were imported books, so Wolf can follow their sales in the book catalogs. He can even list ten owners of *The Independent Whig* in Philadelphia; indeed, in general he makes excellent use of owner inscriptions and marginalia in individual books. But American political pamphlets (which Bailyn estimated at four hundred titles) are curiously absent from this bibliography of Philadelphia books.

Wolf rarely discusses the contents of any of the books

he lists. His book is basically a bibliographical essay, and he admits this in the preface. "I have not drawn general conclusions from the evidence, merely set it down. The historical significance of the books . . . and a socio-economic study of readership I leave to others" (p. vii). Even so, the book is extremely valuable, for Wolf has pointed the way to rich data that await those who study book culture and the meaning of literacy. For example, he briefly mentions the check-out records of 1771–72 of Thomas Bradford's lending library and even prints a sample page, on page 196, showing the titles borrowed on August 12, 1772. The illustration reveals that sixteen of the twenty-seven books checked out on that date were borrowed by women. Typically, Wolf eschews any sort of quantitative analysis of his evidence, preferring to comment impressionistically on the titles checked out repeatedly and on the poor but often amusing spelling habits of the library clerk. But the list certainly invites an analysis of reading by gender.

Many modern-day historians find it hard to keep up with the literature in their own professional subspecialty, such is the onslaught of publication today. Wolf has shown that in the eighteenth century, bookish Philadelphians had easy and quick access to many titles and could easily keep up with the English literary crowd. The number of titles imported was impressive, but it was not so large that it prevented a single individual from reading a good many of them. Wolf's study is a solid starting point for our understanding of the eighteenth-century answer to that timeless and still controversial question: What knowledge is most worth having?

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JÜRGEN HEIDEKING. *Die Verfassung vor dem Richterstuhl: Vorgeschichte und Ratifizierung der amerikanischen Verfassung, 1787–1791*. New York: Walter de Gruyter. 1988. Pp. xli, 1008. DM 398.

The construction of a new political process emerged as the unintended but key result of ratification debates over the federal Constitution. By concentrating on the reception of the document in the states, balancing external influences on the philosophical opinions of the ratifiers against a rising "nationalist" prescription about the republic's needs, Jürgen Heideking offers an exhaustive, European perspective on the Constitution's ratification.

Heideking writes for fellow Europeans, basing his essay on the sources at the University of Wisconsin Center for the Study of the American Constitution at Madison. His invitation from the late Merrill Jensen to use these sources and the nature of an *Habilitationschrift* both show in the book's strengths and weaknesses. This is an immense survey, invaluable as a source book on questions that have been treated in one form or another in the English-language literature. No

other volume brings so many issues and sources and so much historiography together so impressively.

Jensen's influence and perspectives peep through this densely woven curtain. Heideking locates in the state constitutions, the Articles of Confederation, and the protection of fundamental law by Antifederalists major contributions to American constitutional thinking. His fellow West German Willi Paul Adams saw things in a slightly different light in his definitive treatment of the state constitutions, whose pragmatic models, Adams argued, provided the paradigm for the convention at Philadelphia. Republican ideology, the classical traditions, and British precedents in both Adams's and Heideking's analysis seem less significant than the pragmatic rationalism and distaste for the experience of state politics that fired the nationalists' drive for a revolution in government.

Heideking's purpose goes beyond making the issues and historiography accessible to Europeans. He wants to synthesize constitutional history with the social structure of the new republic and the ideological battles that erupted after 1776 over the nexus between society and politics.

The urban centers where the print war over ratification raged provide Heideking with evidence to advance his basic arguments. He deftly summarizes the emergence of public opinion, the *Richterstuhl* of his title, before which battle was joined in the state ratifying conventions. That the Federalists would have lost their proposed document in a plebiscite, despite their appeal to the people at large, no one can doubt, given his evidence.

Heideking's major flaw stems from a tendency to rush the emergence of genuine parties into existence, a claim hard to reconcile with the vicious politics of the 1790s. The extermination of opposition still regarded as wholly illegitimate culminated not merely in the Alien and Sedition Acts but also in Thomas Jefferson's attacks on Federalist "partisans."

Heideking accepts the claim of some scholars that the Antifederalists put no faith in the genuinely revolutionary claims of the nationalists, men who fundamentally reoriented the revolutionary tradition. Yet, while citing Gordon Wood's magisterial treatment of the constitutional period, he never grapples with this seminal work whose major contribution was the bridge it offered between neoprogressive interpretations and older legal-constitutional emphases on continuity and consensus between the British, colonial, and state constitutional phases and the great event at Philadelphia.

Heideking sees the festivities and rituals that hailed ratification as indices of a new national spirit, one that signaled an intention to remain anchored in a trans-Atlantic cultural tradition. The celebrations reflected a restored economic confidence for nationalists, while the same festivities fostered peculiar folk rituals and helped fashion a civil religion out of the political celebrations of the federal nation that Heideking believes marks American popular political culture to this day.

His summary contrasts the pacific transition in America with bloody European failures to bring off such

constitutional changes, an emphasis many Americans of the late eighteenth century pointed to repeatedly. Protection of individual rights and, according to some opponents, states' rights was secured by the eventual amendments with which Heideking closes his account.

Undoubtedly, the constitutional debate was so complex that no dualistic or binary paradigms can ever do it justice. Heideking acknowledges this, but one yearns for a succinct, theoretical statement of how he gets beyond an interpretation of political and social context that transcends a "dialectic between progress and conservation" (p. 869). The "country" suspicion of centralized power and pomp made conservatives less open to a "modernizing" world that some nationalists eagerly embraced. Yet modernization theories used by European interpreters (which Heideking cites extensively) seem less compelling today than ever before. Indeed, the collapse of the first constitutional system in 1861 did more than merely create questions about the capability of the system to evolve solutions to tensions left unresolved by the founders. If the accomplishment of 1789-91 was to make the nature of the state dependent on the quality of the society itself, as Heideking argues, the Federalists labored largely in vain to prevent just this from occurring. Heideking apparently agrees with Wood's latest essays that suggest that the Antifederalists really were the ones who saw the future for what it was.

Cavils aside, this essay deserves an edited English translation to guarantee its reception by scholars of the ratification process. The European perspective along with the exhaustive research reward the steadfast reader of the original.

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JOHN E. FERLING. *The First of Men: A Life of George Washington*. Knoxville: University of Tennessee Press. 1988. Pp. xiii, 598. \$39.95.

John E. Ferling tells us in the preface to this biography that he aspires to be part of the further "humanization" of George Washington. Like Marcus Cunliffe and others before him, Ferling wants to extricate the real Washington from the misty mythos that has enveloped him for so long and restore him to human form. This objective is entirely praiseworthy, even if one duly appreciates at the same time the deep cultural significance of the mythos itself for the people who both created and sustained it. As worthy as Ferling's goal is, however, it is extraordinarily hard to achieve, as much of the historiography on Washington demonstrates.

After all, how does one humanize Washington without debunking him? How does one avoid, in the attempt to make him no more than he was in real life, the fatal trap of actually making him a good deal less? The proof, as always, is in the execution and not the goal.

The quality of the writing in this biography is often excellent. Ferling obviously has a rare feel for the

language. His prose is at times almost poetry, his re-creations of the scenes of Washington's endeavors, especially the natural surroundings in which the surveyor, the planter, and the soldier labored, are gripping and masterful.

Unfortunately, before one has read barely one-tenth of Ferling's study, one begins to encounter noticeable and increasingly serious difficulties. Some apparently typographical errors are easily overlooked, although one such error has John Washington, the progenitor of the clan, coming to Virginia twenty-four years after his famous great-grandson was born there (p. 2). Other errors are factual. At one point, George is one of four sons born to Augustine and Mary Ball Washington (p. 31), but then he is one of only three (p. 58). There are also numerous interpretive inconsistencies, some occurring remarkably close to each other. For example, Washington's decision to enter the military seems intelligible enough to Ferling, because "soldiers still were lauded and lionized throughout eighteenth-century America." A few lines later, however, we are told that many Virginia youths were reluctant to sign up because "this was a farming society, and one with little tradition of active militia service" (p. 24).

One notices some problems of technique as well. For, in the absence of hard evidence, the author too frequently falls back on the "must have been" approach to historical writing. (See, for just some of the many examples, pp. 18-19, 68, 88, 134.) Thus, Governor Robert Dinwiddie's initial reaction on meeting the young Washington "must have been" this and "must have been" that (pp. 18-19). And, since "money and power were the drives that propelled" the rich and corpulent Dinwiddie himself, we are assured that "he must have recognized a kindred spirit in the young man" who was roughly one-third his own age (p. 18). But how does one know such a thing for sure?

The most serious problems concern what we might term "the character question." Many of the author's views of Washington's character are at best highly arguable. As a young military leader, Washington had a "habit of incessant carping," even to prominent superiors like Dinwiddie (p. 46). So Ferling believes. But then he tells us, as though incessant carping were not a flaw, that one of Washington's major traits was to play to his strengths and to conceal his many weaknesses (p. 59). Washington was brave and had magnetism, Ferling allows, but the young commander of the 1750s "seemed unable to harness his ambition, and his lusts led him to excessive absences, to petulant outbursts, to deceitful and irresponsible conduct, to an unsavory manner that vacillated between obsequiousness and a menacing heavy-handedness, and that, at times, verged even on the treacherous" (p. 58).

At this point, the author has simply gotten carried away and fallen headlong into the aforementioned trap, for neither the extant documentation nor the author's own handling of the material revealed therein fully sustains such a sweeping conclusion. Regrettably, most of the limitations to be found in the initial parts of

Ferling's biography persist to one degree or another throughout it.

A blurb on the dust jacket makes bold claims for this book. And there is much to be learned from it, if one uses it carefully as a supplement to, rather than a replacement for, the works of Douglas Southall Freeman, Bernhard Knollenberg, James Thomas Flexner, and many others. But, if one's perception of Washington's character were limited solely to this one book, one would have quite a confused and distorted view indeed.

ROBERT P. HAY

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HARRY M. WARD. *Charles Scott and the "Spirit of '76."* Charlottesville: University Press of Virginia. 1988. Pp. xii, 262. \$24.95.

This first biography of General Charles Scott, a secondary figure of the revolutionary era, should prove useful to military historians of that epoch and to students of the trans-Appalachian frontier. Harry M. Ward makes no extravagant claims for his subject other than to suggest that Scott's life reveals "virtues and vices" in "the character of the new American" (p. 197). (The virtues, it would appear from the text, were public, the vices private.) Nor does the author indulge in large-scale generalizations, except that he attributes Scott's later popularity in Kentucky to a rekindling of revolutionary ardor in the aftermath of Thomas Jefferson's election in 1800. For the most part, Ward is content to let the facts of Scott's public career speak for themselves. Unfortunately, sometimes they do not.

What is rather obvious from those facts is the degree to which Scott's life was governed by the vicissitudes of war. He entered military service at the age of sixteen as a private in the war against France, served for the duration of that contest, was back in uniform in 1775 in the cause of American independence, again for the duration, was involved in the 1790s in the Indian campaigns in the Old Northwest, and then finally played a part in the preliminary stages of the War of 1812. It is not surprising that a large part of the biography is given over to a recital of military events or that the author would regard those military experiences as the determinative influence in the shaping of his subject's character and significance. But, in giving so much attention to that military background, a great deal else of consequence seems to have been sacrificed. Ward makes very little of Scott's background as a Virginia planter of what appears to have been middling rank, a background that surely played a part in shaping his political and social outlook. The thirteen years of Scott's life as a planter before his enlistment in the revolutionary army are dealt with in less than two pages, with nothing more extrapolated from that experience than that "as a gentleman farmer Scott had a good life" (p. 9), a conclusion that hardly prepares us for Scott's decision to take up arms against the mother country. There is a similar lack of an adequate rationale for

Scott's postwar decision to leave Virginia for the Kentucky frontier. The reader is left to surmise that these crucial decisions may have had something to do with financial uncertainties and social aspirations of the sort not uncommon among Virginia's landed gentry. But about such matters we learn very little in this volume.

There is greater attention to background in the chapters that deal with Scott's years in Kentucky. It would have been difficult otherwise to account for Scott's rise to public prominence. With the passage of the years, Ward suggests, the old general came to represent to a new generation of Kentuckians the living embodiment of the "Spirit of '76," a spirit that was particularly congenial to a frontier population intent on having it out with the Indians and, if needs be, with the British. The impression one gathers from the author's treatment of this last period in Scott's life is that he went along for the ride, perhaps because his own private affairs were again in disarray. It might almost be said that Scott entered politics in lieu of another military command.

If this biography is more life than times, it is nonetheless a competent work of reconstruction that adds to our knowledge. Ward has dug deeply into the archives to find out about as much as will ever be known about Scott. If his work is to be faulted it is in a hesitancy to venture beyond the immediate facts of Scott's life into a consideration of their larger implications.

BERNARD FRIEDMAN

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ROGER D. LAUNIUS. *Joseph Smith III: Pragmatic Prophet*. Urbana: University of Illinois Press. 1988. Pp. xii, 394. \$24.95.

When Joseph Smith, Jr., was killed in 1844, there was no clear successor to serve as leader of the Mormon church. Some of Smith's followers chose not to go west with Brigham Young and joined small splinter groups. During the 1850s, several of those groups united, and, in 1860, Smith's oldest son, Joseph Smith III, became president of the Reorganized Church of Jesus Christ of Latter-day Saints (RLDS). According to Roger D. Launius, the career of Joseph Smith III "represents an important case study in the union of principle and pragmatism in American religious history. . . . A practical man of action, he was able to translate his father's religious dreams into some semblance of reality" (p. 369). Smith's adaptation of his father's vision of a utopian society, his reaction to the Utah church's practice of polygamy, and his handling of dissent are examples, Launius contends, of a "pragmatic prophet."

Unfortunately, the book is dependent on Smith's memoirs, written in his old age when he may have been trying to justify his decisions. Most general sources do not include the activities of Smith as a boy. Between the time of his father's death and his ordination as president of the RLDS, Smith lived a private life, and his

memoirs may be the only source on his decision to join the church. Records of his relatives may include descriptions of his trips to Utah, but Launius does not comment on that possibility. Although Smith is the focal point of the study, Launius seems, in many cases, only to present Smith's descriptions.

Despite the weakness, Launius has written an excellent study. Especially valuable are his insights into Smith as a pragmatic leader. For example, Smith questioned several points of doctrine, did not make belief essential for church membership, and allowed discussion of the doctrines he disputed to die out with those church members who had firsthand knowledge of early Mormon church practice. By doing so, Launius shows, Smith attempted to avoid some of the types of disagreements that occurred in the early Mormon movement.

These insights into Smith's character, and especially his reactions to polygamy and the Utah church, are extremely important to understanding the various elements of Mormonism. Although Smith insisted that his father had never practiced polygamy and fought against the practice by trying to convert members of the Utah church and promoting legislation against it, he supported the Utah church's right to religious freedom and Reed Smoot's seat in the U.S. Senate. Launius's explanation of how Smith denied his father's practice of polygamy, by avoiding direct confrontation with those who knew and refusing to accept those who had only secondhand knowledge of it, is especially interesting.

Launius's biography is an excellent study of one religious leader and the effects he had on a nineteenth-century American church. Although the work is slightly flawed by too much dependence on Smith's later memoirs, Launius provides a valuable framework in which to view an important part of the Mormon movement.

JESSIE L. EMBRY

Brigham Young University

F. L. M. PATTISON. *Granville Sharp Pattison: Anatomist and Antagonist, 1791-1851*. (History of American Science and Technology Series.) Tuscaloosa: University of Alabama Press. 1987. Pp. xiv, 284. \$24.95.

As a boy, F. L. M. Pattison heard whispers about Granville Sharp Pattison, the anatomist whose activities as body snatcher and duelist made him a black sheep in the family. Granville Sharp Pattison was the author's great-great-great-uncle, but F. L. M. Pattison's biography is a fair, balanced, and well-researched portrait of a respected anatomist, surgeon, and teacher. Granville Sharp Pattison was also a man plagued throughout his life by bitter quarrels and scandals. While still in his twenties, Pattison was tried for grave-robbery in Edinburgh, found guilty of professional misconduct, and named as "paramour" in a colleague's divorce proceedings.

In America, the exiled anatomist became involved in a pamphlet war, which led to a pistol duel with General Thomas Cadwalader. Nevertheless, Pattison became a respected faculty member at the University of Maryland where he was influential in the establishment of one of the first modern residential teaching hospitals in the United States. After a dispute between the medical faculty and the state legislature, Pattison accepted a professorship at the new University of London.

Conflicts with colleagues and administrators were to be expected, considering Pattison's previous record. The London years, however, were most memorable for the hostility and open rebellion of his students. Dissident students publicly accused him of being incompetent, incomprehensible, and boring. All teachers will sympathize with Pattison's frustration at being attacked by some students for failing to lecture about a particular topic, while others complained that he had spent too much time on the same topic. The administration retreated in the face of student unrest and forced Pattison to resign. Generally, the author insists that Pattison was an excellent and popular teacher. He fails to provide a compelling explanation for Pattison's difficulties with students at the University of London.

Shortly after his dismissal, Pattison was appointed professor of anatomy at Jefferson Medical College in Philadelphia. Here Pattison was accepted as an excellent lecturer. J. Marion Sims remembered Pattison as eloquent, sympathetic, and charming, despite a tendency to "splutter and slobber and spit" in his enthusiasm (p. 18). Surprisingly, the American students were so polite and well behaved that they waited until Pattison left the room before wiping away the spit. Given the riot and rebellion of the London classroom, some analysis of this remarkable difference between Pattison's American and English medical students should have been attempted.

After joining the faculty of the new medical school at New York University, Pattison enjoyed ten relatively tranquil years. In the last years of his life, this master of pre-anesthetic surgery seems to have become more sensitive to and distressed by the pain he inflicted on his patients. Of course, Pattison was not a major medical or scientific innovator, but his turbulent life provides a fascinating view of the medical and academic world of the first half of the nineteenth century.

LOIS N. MAGNER
Purdue University

ELBERT B. SMITH. *The Presidencies of Zachary Taylor and Millard Fillmore*. (American Presidency Series.) Lawrence: University Press of Kansas. 1988. Pp. xi, 302. \$25.00.

According to Elbert B. Smith, "no two presidents have been more victimized by history than Zachary Taylor and Millard Fillmore" (p. 257). They place near the bottom of every rating of American presidents and have long been discounted or even ridiculed. Smith's

book is a revisionist attempt to correct that traditional interpretation and to rehabilitate the respective reputations of the twelfth and thirteenth presidents.

Like other volumes in the American Presidency Series, this book presents a concise, interpretive account that covers the most important political, diplomatic, and economic questions of the Taylor and Fillmore administrations and the period from 1848 to 1853. It is well organized, clearly written, and tightly argued. Based on secondary works and printed primary sources, the study reflects the author's extensive knowledge of the personalities and politics of the 1840s and 1850s. Although it includes an excellent bibliographical essay, the footnotes are uneven and include citations from college textbooks as well as primary sources, historical monographs, and biographies.

According to Smith, each man was well qualified in different ways for the presidency and executed the duties of the office in a capable manner. The author notes that Old Rough and Ready was not adept at dealing with Whig factional rivalries or the patronage struggles and acknowledges that he did not provide effective leadership to a Congress dominated by proud and powerful figures in both parties, such as Henry Clay, John Calhoun, Daniel Webster, and Stephen Douglas. Nonetheless, the author gives Taylor high marks for the conduct of his administration and for his handling of foreign policy. In regard to the Compromise of 1850, Smith praises Taylor for his strong anti-Texas stand on the border dispute with New Mexico and argues that his death did not pave the way for the eventual passage of the Compromise of 1850. Indeed, the compromise might well have been approved had Taylor lived, according to Smith.

Fillmore receives similar sympathetic treatment. Smith clearly admires Fillmore's character, talents, and high-minded intentions. Like his predecessor, Fillmore "deserves high praise for his leadership as a maker of foreign policy" (p. 233). Moreover, the author takes sharp issue with those historians who have argued that approval of the Compromise of 1850 resulted from the death of Taylor, who espoused antisouthern views, and his succession by Fillmore, who had prosouthern sympathies. In fact, "historians have overemphasized the sharp break between the Taylor and Fillmore administrations and have ignored the continuity" (p. 168). According to Smith, the most important change was the recasting of personal enmities produced by Taylor's death, not a substantial change in the domestic policies of the president.

Although Smith acknowledges that neither Taylor nor Fillmore achieved greatness, he argues that "both were, in fact, superior presidents when rated by standards most Americans would accept as important" (p. 263). Each studied the issues, reached independent and reasonable conclusions based on the common good, and carried out his constitutional responsibilities in an honest and forthright manner. Historians of the antebellum period have much to consider and to dispute in these pages. Disagreements are sure to be

expressed on numerous specific points of interpretation. Like other presidents (and leaders) of the period from 1849 to 1861, Taylor and Fillmore were unable to control the formidable forces and problems they confronted. Although they may have been well-intentioned and capable caretakers of the presidency, it is unlikely that historians will accept the judgment that either was a "superior" president. At the same time, Smith is to be commended for rejecting hackneyed treatments of his subject and for presenting strong judgments and fresh interpretations of these important events in American history.

JOHN H. SCHROEDER
University of Wisconsin,
Milwaukee

SALLY MCMURRY. *Families and Farmhouses in Nineteenth-Century America: Vernacular Design and Social Change*. New York: Oxford University Press. 1988. Pp. xiii, 261. \$29.95.

This original and many-faceted excursion into rural history demonstrates how a conjuncture of current social issues can promote novel ways of reinterpreting the recent past. Sally McMurry wants history to serve as a resource in understanding the social consequences of the capitalist restructuring of rural America. Her epilogue, which enunciates this purpose, is timely. The contemporary context for her work is surely the demise of the family farm. Anyone who grew up in a farm community and retains contact with it has an awareness of a long-brewing crisis, of a dissonance between agrarian mythology and the involvement of corporate and state power.

For McMurry, substantial credit for today's business of agriculture belongs to the progressive farmers of the nineteenth century. Like the contemporary Yankee artisans who sometimes became prototypical industrialists, these individuals, too, were a force for change. History is often the study of unanticipated events, and these agents of progress might now be surprised by what their initiatives did to the values they cherished. Principally, McMurry formulates a familiar theme of a loss of organic social coherence as a result of the triumphant spirit of enterprise. This time, of course, the setting is neither the factory nor the city but the townships of the North and the Midwest. McMurry often measures the impact of capitalism in terms of gender-based analyses of farm work and one artifact related to that work, that is, the house.

Rural social history as a treatment of housing comes as a refreshing counterpart to the achievements of urban history. It is fitting, too, that McMurry acknowledges Paul Gates, a pioneer of rural social history. The gender aspect is a not unexpected advance in the historiography of rural America. Farm women have been receiving some recognition of property rights in the courts for their contributory labors, at least in Canada. And can history be far behind? In sum,

the book is well grounded in historiography and topical issues. McMurry's sources and methodology may be thought somewhat avant-garde. In truth, she used largely conventional primary material and made a thorough search of the secondary literature. She also, however, looked at house plans submitted to farm journals by progressive farmers; she has attempted to analyze these, relating innovations back to the restructuring of farm economics as well as to gender-based divisions of labor. The latter, she proposes, serve as highly sensitive and pertinent social indicators. Thus, when progressive farmers designed farms to facilitate production and when production meant bulk production with less farm-based refinement (for example, the sale of milk to a cheese factory instead of cheese production on the farm), women were removed from household production. Consequently, kitchens became more "domestic."

As competitive individualism mounted, house designers incorporated more privacy into their dwellings, for the collective endeavours of farming and the spirit of community were being replaced by a more business-like approach. The dynamics of interior arrangements, McMurry suggests, were complex and cannot be done justice in a review. An example will suffice. She believes that a considerable, but diminishing, informality in the farm dwelling led to the decline of the parlor in the city. Rural migrants brought their casualness into town. There may be other contributing factors. Here and elsewhere she may not have the complete story, but, by setting up and illustrating fine hypotheses, she has staked out a base for inquiries into social history and specifically the spatial relationships within dwellings.

A synopsis within a review misses many interesting details, although one hopes to convey the novelty of a book. Beyond the links that this study establishes with conventional writing, it makes original connections. Novelty has its risks. In the first chapter, before purpose, themes, and style can register clearly in a reader's mind, direction seems unsteady. Occasionally, observation of gender appears problematic. It is not easy to accept McMurry's proposition that farm technology lightened male labor and left female chores as uniquely ones of drudgery. Much of all farm labor was and remains tedious; mechanization often has increased the risk of injury. Of course, she focuses on the house, not the barn or the field. Let it be said that farm women toiled in those places, too, and then returned to a separate sphere of domestic tasks. McMurry is wisely cautious when dealing with another element of women's work, namely, whether women retained control of earnings from "kitchen industry."

Farm journals can suggest only the outlines of probable patterns of drudgery and domestic management; they are not the record of the routine realities of general conduct, those elusive concerns of social historians. For that reason, McMurry's focus on "progressive farmers" was a necessary and legitimate tactic. If novelty and creativity have their risks, then let it be said, first, that McMurry has coped with them and,

second, that able young scholars ought not be deterred from sticking their necks out. Good new insights, like the ones found here, enhance our craft.

JOHN C. WEAVER
McMaster University

KENNETH J. WINKLE. *The Politics of Community: Migration and Politics in Antebellum Ohio*. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1988. Pp. xiii, 239. \$32.50.

The United States was transformed early in the nineteenth century when millions of people moved to new farms or towns in the trans-Appalachian West and when existing communities grew much larger. A number of scholars, including Stephan Thernstrom and Peter Knights, have studied northeastern cities during this period and have discovered that the cities had staggeringly high rates of family geographic mobility. All but the most successful urban families moved repeatedly, producing such social churning that community stability seemed threatened. Although historians of the trans-Appalachian West have paid less attention to mass mobility, Merle Curti, Andrew Cayton, and others have studied the process of community formation, noting the rapid appearance of enduring elites in new towns. Stanley Elkins and Eric McKittrick have suggested that dominant families in new communities cooperated with each other primarily to make their towns more prosperous or powerful than neighboring towns. The early West was clearly shaped by rapid growth, and its social and political developments remain important areas for research.

Kenneth J. Winkle investigates the importance of mobility in an important antebellum state that contained northern, southern, and European settlers. He compares census and tax records with surviving annual election poll books and proves that Ohio communities had much the same mobility as seacoast cities. To be sure, every county and township had a unique socioeconomic pattern, since each was settled at a different time, possessed different soils and terrain, and went through economic transformations shaped by the presence or absence of canals or railroads. Nonetheless, Winkle demonstrates that almost every area had a steady out-migration rate that removed 60 to 75 percent of the voters every decade, while mortality took others from the community. Newcomers replaced those people for a few years and then themselves moved on, to be replaced by other transients. Many men migrated annually from job to job. Some counties grew despite out-migration but only because they attracted an unusually large number of transient immigrants. In a typical county, a small core of enduring residents, often the most prosperous men, exercised political dominance over a much larger pool of migratory farmers and townspeople.

The Whig and Democratic organizations in every county had to mobilize new resident voters in every

election, bringing their supporters to the polls to vote for established local leaders. Ohio at first erected a series of barriers to voting, based on sex, age, race, citizenship, property ownership, and residence, that disfranchised many migratory men as well as women, nonwhites, and most aliens. General community consent often determined resident status. By the 1850s, reform laws eliminated most property requirements, and the state adopted new residence laws permitting potential voters themselves to declare whether they thought that they were members of a community. Thus, community consent to membership was in theory replaced by voter volition. In practice, as Winkle demonstrates by reviewing a well-documented contested election, partisan election judges readily let their followers vote but interpreted remaining laws on length of residency to disfranchise or discourage supporters of the opposition party. Mobility thus remained an important political factor in every election.

Migratory voters had little power. Prosperous and entrenched men in every county established local political control, and narrow statewide election margins only partially masked the dominance of the local organizations. In that fashion, Ohio's leaders maintained order in the wake of sweeping change.

Winkle has produced an impressive and persuasive assessment of the impact of mobility on Ohio politics. The migratory majority of families in the state conceded political influence through their repeated quests for economic success in new communities. Based on scattered studies of other states, Winkle asserts that the same conditions held true throughout the Great Lakes frontier. That may well be the case, but it needs to be proven through further research. Winkle's work clearly proves the extraordinary degree of family mobility in antebellum Ohio and demonstrates that scholars must assess persistence and migration when they study social and political evolution in the rapidly developing trans-Appalachian states.

JEFFREY P. BROWN
New Mexico State University

J. SANFORD RIKOON. *Threshing in the Midwest, 1820-1940: A Study of Traditional Culture and Technological Change*. (Midwestern History and Culture.) Bloomington: Indiana University Press. 1988. Pp. xiii, 214. \$35.00.

Midwestern threshing underwent significant changes in the century after 1820. The act of detaching grain from stalks, threshing was earliest accomplished by flail or by treading with horses. It was in either case a relatively solitary enterprise. Families generally threshed alone without the assistance of neighbors. By 1880 the situation had changed markedly. Steam engine-powered threshers began to dominate the region and threshing rings—neighborhood groups of six to twelve families—worked collectively; threshing became a communal activity. It remained so until the eve of World War II. Only then did individually owned and

operated gasoline combines supplant the communal steam thresher.

J. Sanford Rikoon's study is best when cataloguing these threshing technologies and discussing the social dynamics of threshing rings. Indeed, the author devotes more than half of the book to the neighborhood groups, recognizing the rings as important social institutions during a period in which rural churches and schools seemed to be failing. The study is less sure when the author examines the nature of and reasons for change. A sociologist specializing in rural culture, Rikoon identifies availability of new threshing technologies as the determinant of change and makes a special plea to members of his discipline to measure the social as well as the economic consequences of these technologies. His is a technology-driven taxonomy in which neighborhood reorganization and the institution of new activities are principally, if not solely, the result of the mechanization of threshing. In his schema, Rikoon conceives of each successive technological device as a social force and revamped social arrangements as virtually inevitable responses to those forces. This predictive, cause-and-effect model constrains, leaving no room for cultural or other human variables. It ignores the fact that technological devices are the consequence of human thought as is their adoption. In that sense, the volume is curious. It is stocked with anecdotes from real people—Rikoon relies heavily on interviews—but denies those people the ability to modify their lot, while absolving them from any complicity in its formulation. People in this history are incidental, merely the beneficiaries or victims of the ineluctable force of a new technology.

ALAN I MARCUS
Iowa State University

KENNETH SEVERENS. *Charleston: Antebellum Architecture and Civic Destiny*. Knoxville: University of Tennessee Press. 1988. Pp. xiii, 315. \$49.95.

In this handsomely illustrated volume, Kenneth Severens studies Charleston's public buildings "as cultural symbols that expressed civic realities and aspirations" (p. xi). He argues, in the manner of American studies, that Charleston's architecture veered after 1820 from the prevailing nationalistic design common throughout the early republic to styles that better expressed the city's unique destiny as "the center of southern nationalism" and that asserted an "aggressive regionalism" rooted in "the peculiar characteristics of the South" (p. 23).

That the city, as a former state capital, already had in 1820 a surfeit of government buildings but nonetheless continued to build more is, for Severens, a sign of its progressivism (alas, a term he never defines), expressing "the salutary relationships among plantation society, commercial hegemony, and civic institutions" and disguising "the troubling aspects of a city based on a slave economy" (p. 24).

This approach to nineteenth-century American urban architecture promises, however, rather more than it delivers. First, considering that in a forty-year period fashion raced from Roman to Greek to Gothic to Moorish and then melded in a new Victorian eclecticism, Severens's effort to link each style to specific political or economic events fragments cultural history unrealistically in periods sometimes as short as three years. Second, his faulty command of social, economic, and political history results in unfortunate distortions. The rebellion of students at the College of Charleston in the early 1830s, for instance, like the medical school faculty's revolt against control by the local medical society, cannot adequately be explained by nullification, for similar collegiate and professional controversies raged simultaneously in the North. Consequently, Severens's political explanation of Charleston's buildings is unconvincing. It is hard to believe that it was the enthusiasm of Charlestonians for innovative design that eliminated white laborers' antipathy to out-of-state artisans just when bad economic times gave way to good. More specifically, attributing the bankruptcy of former governor James Hamilton to overinvestment in city improvements rather than to his failed attempt to corner the cotton market and his speculations in Texas lands is to misunderstand altogether the economic plight of a city whose commercial capital continually was siphoned off to agricultural investment.

Nonetheless, beyond his perceptive analysis of individual antebellum architects and their work, Severens does make clear that the residents of Charleston were patrons of architectural innovation in public structures. Moreover, Charlestonians' dedication to ecclesiological architecture, the doubling of the city's churches from fifteen to thirty-three in a forty-year period when the total population grew by less than three thousand, and the building of two churches whose very structures challenged racially segregated worship expressed a complexity of values as relevant then as now. Furthermore, Severens's examination of school and industrial buildings raised in the 1850s, like his elaboration of functionalism and cast-iron technology used in commercial structures of the same decade, puts the Charleston of *Gone with the Wind* in a new light. And those explorations lead to his gloss on urban leaders' enthusiasm for Victorian brownstone—an enthusiasm so great that, in the immediate pre-Civil War years, they planned an architectural renewal, a remodeling of old-fashioned buildings to display uniformly up-to-date facades. This analysis is, indeed, Severens's best testament to the interaction of cotton-boom economics, secessionist politics, and architecture. Had Charleston realized its aspirations to become the capital city of the South, it might have mimicked Georges-Eugène Haussmann's Paris or Vienna's Ringstrasse. But other realities preserved the colonial and federal cultural symbols to which late twentieth-century tourists flock.

JANE H. PEASE
College of Charleston

LAWRENCE LEE HEWITT. *Port Hudson, Confederate Bastion on the Mississippi*. Baton Rouge: Louisiana State University Press. 1987. Pp. xvi, 221. \$19.95.

Sieges have lasting fascination. The battle of Vicksburg claims the most renown of the Civil War sieges, but that reputation is not entirely fair. About one hundred miles south of Vicksburg and less than twenty miles north of Baton Rouge stands the ghost bastion of Port Hudson, the site of "the longest genuine siege in American military history" (p. xi).

Construction began at Port Hudson in April 1862, and by August the fortress filled with Confederate troops. Along with Vicksburg, Port Hudson made possible a rebel hold on the Mississippi River and tied the Confederacy together. Like its counterpart upriver, Port Hudson was neglected by both sides throughout 1862. As Union efforts to divide the South intensified, Ulysses S. Grant fixed on Vicksburg and General Nathaniel P. Banks concentrated on Port Hudson as objectives. Admiral David G. Farragut tried to help Banks flank Port Hudson and perhaps force evacuation by controlling the mouth of the Red River. When this plan failed, Banks determined to carry Port Hudson by storm. He attacked in full earnest on May 27, 1863, while Grant was besieging Vicksburg. The attack should have succeeded and would have saved for timidity and delays that gave defenders an extra chance for readiness.

Defenders had varied problems. Major General Franklin Gardner's determination can be measured by his comment that "the enemy are coming, but mark you, many a one will get to hell before he does to Port Hudson" (p. 131). But his words seem hollow, considering that his 7,450 men (20 percent unfit for duty) had to hold about eight miles of works against nearly forty thousand Federals. Still, Confederates beat off a major assault on May 27 and clung to their bastion until July 9, five days after Vicksburg surrendered. At the end, Gardner had only 3000 men standing for surrender, but those stalwarts had inflicted five thousand casualties while sustaining only five hundred. Sadly, such gallantry counted for little in the great scheme of the war, but it counts for much in memory.

Lawrence Lee Hewitt tells the story up to the attack well, describes graphically the long days' testing, and then lets his story fade too soon. The rest of the siege deserves attention. Instead of telling his whole tale, the author launches into a thesis that Port Hudson was decisive because of two considerations: one, that Banks was probably prevented by the length of the siege from receiving supreme command of the Union forces and, two, that the commendable conduct of black troops at Port Hudson led to their increased, perhaps irresistible, use in blue ranks. Both assertions are dubious.

This is a flawed but useful book.

FRANK E. VANDIVER
Texas A&M University

DURWOOD DUNN. *Cades Cove: The Life and Death of a Southern Appalachian Community, 1818-1937*. Knoxville: University of Tennessee Press. 1988. Pp. xvi, 319. Cloth \$24.95, paper \$12.95.

No image of Appalachia has been quite so durable as that of an isolated, backward and static society cut off from the main currents of American life. Whether portrayed as a neo-frontier, a remote backwater, or a resource colony, the southern mountains have been cast in the popular mind as part of that "other America," a region "within" but somehow not "of" America. This book is the latest in a series of studies that challenge that image and seek to reconstruct the mountain experience. Concentrating on the life of a single community in the Great Smoky Mountains, Durwood Dunn questions many of the conventional assumptions about preindustrial life in Appalachia, and, in so doing, his study contributes to the broader understanding of the history of rural life.

For many Americans, Cades Cove is a picturesque tourist attraction within the Great Smoky Mountains National Park. Selectively preserved cabins and other structures present a stereotyped and romantic version of southern Appalachian culture, a living museum captured in time by the park's creation in the 1930s. Dunn, however, delves beyond this traditional image of mountain life and traces the history of the community from its settlement through its demise by eminent domain. The direct descendant of the first permanent white family to settle in the cove, Dunn draws heavily on a remarkable collection of diaries, church records, sketches, and other memorabilia that he inherited. Combining these sources with store receipts, tax records, deeds, census data and other public records, he describes a community that was more diverse, more fluid, and more closely connected to the larger society than the popular image would have us believe.

In contrast to other accounts of the Appalachian frontier, Dunn finds that the wilderness did not impede the development of an organized society and of an agriculturally based market economy in Cades Cove. From the first white settlement in the 1820s, the fertility of the soil "tied the people closely to regional markets, and through these markets, to the broad mainstreams of American political and social culture throughout the century" (p. 88). Land and a strong sense of community helped shape the character of life in the cove, giving the community a greater sense of self-sufficiency, but that self-sufficiency was communal, not individual, and was always dependent on external political and economic conditions. Indeed, it was the very nature of those connections to the external society, not its isolation, Dunn concludes, that determined the evolution and eventual death of the community. The cove prospered and grew in the decades before the Civil War but declined in the later decades of the century as a result of the general depression of the postwar economy in all of eastern Tennessee and the larger region.

This study offers the first detailed analysis of a remote southern Appalachian community in the nineteenth century. It should lay to rest older images of the region as isolated and static, but it raises new questions about the nature of that premodern community. Dunn, for example, points to the existence of both an "outwardly market-oriented" (p. 89) economy and a strong internal economy shaped by shared values, self-sufficiency, and collective responsibilities, but he fails to explore the relationship between these two economies, how they changed over time, or the degree to which the market economy affected the lives of diverse farmers within the cove. Having determined that Appalachian people "followed regional and national patterns of development" (p. 256), we need to broaden our understanding of why those patterns produced a distinctive path to modernization in the region and why, in some rural areas, as in Cades Cove, they led to the community's demise.

RONALD D ELLER
University of Kentucky

THEODORE R. MITCHELL. *Political Education in the Southern Farmers' Alliance, 1887-1900*. Madison: University of Wisconsin Press. 1987. Pp. xiv, 242. Cloth \$39.50, paper \$16.50.

The agrarian movement billed itself as a grand educational crusade, but until now the history of Populism has received little attention from historians of education. Theodore R. Mitchell has changed that in an intriguing treatment of the "southern" Farmers' Alliance. Taking a cue from Lawrence Cremin, Mitchell situates the Farmers' Alliance within a configuration of educational apparatuses through which culture was transmitted. And, following the insights of Paolo Freire, he views both the "curriculum" and the "pedagogy" of the Alliance as a means of disseminating an alternative culture.

The Alliance curriculum was a critique of the orthodox political economy and Whiggish history of contemporary textbooks, a critique that drew selectively on familiar traditions of "producerism," evangelical stewardship, and the history of working people. In reconstituting this curriculum, Mitchell relies on the writings of Alliance leader Charles W. Macune, particularly as expressed through his newspaper, the *National Economist*.

Alliance pedagogy was practiced in the local assemblies of the organization, called suballiances, by traveling lecturers and local officers. In addition, the Alliance's newspapers, in particular the *National Economist*, disseminated "educational exercises"—serialized lessons on government, political economy, and history, which conveyed the doctrines of the movement.

Mitchell makes his greatest contribution by following Freire's lead in describing and speculating on the convergence of curriculum and pedagogy. Through its network of lecturers, the Alliance reached illiterate

rural people, and, through the suballiances, the organization became an institution of adult education, teaching literacy and mathematics along with political economy. (Mitchell infers this from a close reading of Alliance documents rather than from case studies of actual suballiances.)

A comparison with the literacy and political education campaigns of Latin America and the Caribbean—Cuba in particular—enriches the study of American Populism. The Alliance's "schools" created an alternative educational system and helped disseminate an alternative culture. That educational system clashed with the expert-based crusade for public schools in the South and with the "text-book trust," both of which, Alliance members believed, were instruments of an oppressive dominant culture.

Although this book contains intriguing new ideas, its shortcomings reveal something of what is still to be done to understand the workings of grass-roots political education. First, Mitchell needs to go further in situating the Alliance within an alternative educational "configuration." Its system of lecturing was part of a familiar pattern of associational activity ranging from Masons and Methodists to the Knights of Labor. Second, the book conveys an overly monolithic view of the economic, social, and political context of the Alliance movement. The "southern" Alliance was not solely a creature of the cotton belt (nor, for that matter, of the South), nor had the Democratic party by the 1880s developed such an iron grip on local politics in the South as Mitchell suggests. Finally, among agrarian and labor leaders of the early 1890s, "education" was often a code word for nonpartisan political activity. It is not clear how many of the educational initiatives in the critical period of 1891-92 that Mitchell describes were last-ditch efforts of Macune and his associates to stave off the formation of a third party.

These limitations are outweighed by the book's new insights. By taking seriously the educational program of Macune and by connecting it to the Latin American tradition of an alternative pedagogy of the oppressed, Mitchell offers a promising clue about the connection between the pervasive, but often unself-conscious, republicanism of nineteenth-century rural Americans and the "movement culture" of Populism that seemed, to some, to appear *de novo*. There is still much to be learned about how that connection was actually made at the grass roots.

ROBERT C. MCMATH, JR.
Georgia Institute of Technology

CATHY L. MCHUGH. *Mill Family: The Labor System in the Southern Cotton Textile Industry, 1880-1915*. New York: Oxford University Press. 1988. Pp. x, 144. \$24.95.

This study by Cathy L. McHugh, an economist, focuses on the "recruitment and utilization of a factory workforce for the southern cotton textile industry between 1880 and 1915" (p. 3). McHugh makes extensive use of

the available records from the Alamance Mill in Alamance County, North Carolina, in particular, payrolls (1890 to 1912) and employee rental housing records (1908 to 1920). She also uses other Alamance holdings, the manuscript censuses of 1900 and 1910, census reports, the U.S. Commissioner of Labor's reports on the conditions of women and child workers, North Carolina state labor reports, and a number of secondary works.

McHugh examines, sometimes using econometric methods, the unusual but not unique family labor system employed by the southern cotton textile industry. The system, she contends, helped the industry to recruit, train, and retain a work force that was largely drawn from displaced farmers. The system was reinforced by the paternalism of management, which operated company stores, subsidized or completely funded schools, and subsidized churches and ministers. According to McHugh, child labor was necessary because it increased the ability of the mills to attract families as units and because families needed the income from their children's labor.

McHugh also determines that worker earnings depended more on experience and gender than on education or age. Men earned more than women. That discrimination was exacerbated by the exclusion of women from supervisory positions and the better-paying skilled jobs. The lack of a positive correlation between education and earnings probably reflects the state of technological development of the textile industry in the South at the time and may also throw some light on the indifference, sometimes hostility, that mill worker parents displayed for some years toward education for their children. As late as the early twentieth century, mill parents rejected schooling for their children as an interference in their lives and as a threat to their family income.

McHugh's contention that the family wage system fit the circumstances of the southern textile industry and helped meet its needs to recruit, train, and retain workers seems entirely correct. It was an industry in its early development stages, and it grew up in a rural, small-town South that had a large number of displaced farmers. Indeed, she might have noted the impact of the devastating losses of the Civil War on small farmers, landed and landless. The war may help explain why so many of the mill families in the 1880s and 1890s were headed by women.

McHugh's contention that child labor was necessary either as a recruiting device or as a source of family income is not, however, persuasive. Management, as she herself indicates, could simply have raised wages for adult workers and obviated either of those needs. Management's suggestions that it wanted to get rid of child labor but could not because of worker resistance are not plausible. Management was hardly that vulnerable to worker pressures and took a long time to reduce its use of child labor significantly and only reluctantly acquiesced to child labor legislation or to compulsory school attendance laws. Very likely, the more plausible

argument is that management found that child labor served its interests and retreated from that position only when it perceived its interests differently. Child labor, as McHugh says, served the needs of the industry to train workers, particularly in the early years of the industry.

Other aspects of the book are troublesome. In part, these aspects are the result of the limited primary sources available to scholars of the southern textile industry and the small, though rapidly developing, number of scholarly works on the industry. The Alamance records represent an unusually rich source of quantitative data. It is difficult at times to tell when McHugh is dealing with Alamance or with a larger setting. That tendency is complicated by sweeping generalizations, which seems odd for this type of study. Discussion of the "comprehensive welfare system of the southern cotton mill village" (p. 47) needs more facts and more attention to change over time than are provided here. Some large assumptions about management's paternalism and control are not tenable in light of recent scholarship. Very likely, Edgar Gardner Murphy, the Alabama Episcopal minister turned child-labor reformer (not "Edgar Murphy, a philanthropist" [p. 43]), would have taken exception to some of McHugh's assumptions about management.

The book is a useful study and adds in important ways to a major area in the history of the American South. Economists have made particularly significant contributions to that history and, not infrequently, have corrected serious errors of historians. We are in their debt even when we continue to find that econometrics confirms our worst suspicions about the "dismal science."

THOMAS E. TERRILL
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Columbia

HENRY C. DETHLOFF. *A History of the American Rice Industry, 1685-1985*. College Station: Texas A&M University Press. 1988. Pp. xiii, 215. \$29.50.

Henry C. Dethloff's study of the American rice industry is basically two stories, that of the labor-intensive Carolina rice culture, which began during the seventeenth century and declined after the Civil War, and that of the capital-intensive prairie culture, which began in Louisiana during the 1880s and has continued to expand. The author focuses on the rice industry, "the middlemen—the traders, brokers, agents, bankers, millers, and shippers," and he faults other historians for concentrating on "the plantation South and on rice culture on the plantation" (p. 25). His primary contribution is tracing the business aspects of the rice trade in both the Carolina and the prairie cultures. By focusing on milling and marketing and neglecting recent scholarship on agricultural labor, however, Dethloff misses the significance of his brief remarks that slave labor was used in rice mills and that free

labor contributed to the fall of the Carolina rice culture after the Civil War.

Dethloff traces the history of milling technology and uses Jonathan Lucas of Charleston as an example of its development. There are tantalizing glimpses of the use of tidal flow to drive mills and of the later application of steam power. The use of such advanced technology in processing southern commodities, especially rice, sugar, and cotton, raises key questions about the technological "backwardness" of the South.

Dethloff dedicates two-thirds of his book to the history of the prairie rice culture, with a brief discussion of the development of rice culture in California. His analysis centers on the role of the Rice Millers' Association, whose papers are the basis for Dethloff's research for the final two-thirds of the book. That allows Dethloff to explore the development of the milling industry both in New Orleans and in the Louisiana prairie and to chronicle the rice trade with Puerto Rico and Cuba.

Although Dethloff offers a brief treatment of the development of the prairie rice culture and discusses the origin of pumps, canals, and early methods of prairie farming, he skims over later developments. He takes little notice of the innovations in binders, traction engines, threshers, tractors, and combines. Because such farm implements were crucial in defining how farms and communities organized during harvest and threshing, discussing them could have clarified how rural life changed over time and how the crop cycle and technology dictated rural life. Prairie farmers applied the latest technology not just to processing but also to field work because the prairies, unlike the Carolinas, lacked cheap labor and because prairie settlers were familiar with the technology of wheat farming. It is perhaps this sketchy treatment of farm life and its changes that prompts Dethloff to conclude on the last page of the book, "Rice growing in America has been distinctly an agribusiness, as opposed to a 'way of life' on a family farm. Although rice farms were family operations, they have been heavily capitalized" (p. 194). Is there a contradiction between farming as a way of life and capitalism? Given the recent scholarship on the development of commercial agriculture, Dethloff's statement demands clarification.

The author concludes by assessing the "sophisticated infrastructure" of the present rice industry, which, he argues, "is no longer simply producers, millers, and merchants, but these *and* governmental and educational agencies linked through business, trade, and political associations" (p. 194). The rice industry has indeed grown into a giant, but the conflicts generated within the infrastructure are not explained. Dethloff leaves the reader with unraveled threads and an unsatisfying argument.

PETE DANIEL
National Museum of American History
Smithsonian Institution

JAMES MICHAEL RUSSELL. *Atlanta, 1847-1890: City Building in the Old South and the New*. Baton Rouge: Louisiana State University Press. 1988. Pp. xiii, 314. \$32.50.

James Michael Russell's new book analyzes the reasons behind the rise of Atlanta, through fire and sword, from a railroad junction point to a growing regional metropolis, all within the span of only four decades. Russell contends that Atlanta's formative years set the city on a course that led to metropolitan greatness. Hence, Atlanta's early period has implications for the present day. "We can better understand the historical forces that still have an impact on this city of more than two million people (in 1980), the sixteenth largest standard metropolitan statistical area in the United States," Russell explains. "Although there will be no attempt to depict Atlanta as representative of the entire South, its history, if for no other reason than the city's economic and political importance, provides valuable insights into the region as a whole" (p. 12). Although this conclusion is so broad that it can apply to any successful American city, it points in the direction of explaining the rise of Atlanta in terms of successful entrepreneurial leadership.

Russell, according to his introduction, has been influenced by the work of other pioneer southern urban historians, including David Goldfield and Don Doyle, who have played down economic equations in southern city building. Russell, however, goes off on various tangents, which makes for a muddled analysis and some conclusions that strengthen conventional wisdom. Painstaking and detailed research shows that Atlanta business leaders were not necessarily self-made men, that antebellum commercial values related directly to those in the "New South" era, that most of the pioneer builders of Atlanta were natives of the South, and that southern interior upland city leaders had a more aggressive outlook than their counterparts in the older seaport cities. Tedious sections on urban services and politics provide standard fare, without placing developments within the context of a city passing through a necessary adolescent stage, when rapid growth outstripped the tax base and the machinery of urban government in Gilded Age cities. This omission points up a major limitation of Russell's monograph.

By concentrating on answering the needs of a rather parochial group of southern urban historians, Russell has avoided placing his book within a national context. As a result, he ends up going on the defensive, proving obvious truths in a book that could have paved new ground by showing relationships, if any, between Atlanta and other nineteenth-century interior cities, such as Chicago, Kansas City, and Denver. Still, despite limitations, Russell has made an important contribution by demonstrating that the impulses that led to the urbanization in the nineteenth-century South were similar to those throughout the rest of the nation.

LAWRENCE H. LARSEN
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ROBERT P. INGALLS. *Urban Vigilantes in the New South: Tampa, 1882-1936*. Knoxville: University of Tennessee Press. 1988. Pp. xx, 286. \$29.95.

Framing this book between two lynchings, Robert P. Ingalls uses the history of Tampa, Florida, to illustrate his thesis that vigilante activity in the United States "was in every sense establishment violence" (p. 206). In his well-documented view, community leaders established and led vigilante groups when people and organizations that threatened the status quo "broke no formal law and, therefore, could be repressed only outside the law" (p. xvi). Tampa's elite used vigilance committees in a series of clearly defined class struggles during the community's growth from frontier town to major city.

In his first chapters, Ingalls outlines the roots of vigilantism in southern plantation ethics. "Prominent among these mores was the concept of honor, which sanctioned community justice—including lynch law—to defend purity, self-esteem, property, and white supremacy" (p. 30). Thus, business leaders, city officials, and lawyers directed the 1882 lynching of a transient who raped a white woman. In fact, leaders of lynch mobs over the next decades rarely even bothered to conceal their identities.

This code developed into a community standard that accepted antiradical violence in Tampa's dominant industry, cigar making, from the late nineteenth century on. "To counter the legal activities of cigarworkers, prominent Tampans relied on their vigilante traditions to protect their interests, which they identified with those of cigar factory owners" (p. 31). After a strike in 1887, a so-called Committee of Fifteen ordered nine union organizers to leave town. Tampa's highest elected official led this extralegal mob that, like later vigilantes, blamed conflict on a few "outside agitators." For example, the *Tampa Tribune*, a consistent proponent of lynch law, considered local Cuban and Spanish cigar workers normally docile. But, wrote the *Tribune*, "when subjected to the devilish influence of even one unprincipled socialist, communist or anarchist, they are transformed into little less than madmen" (p. 52).

Despite such beliefs that only a few troublemakers needed to be weeded out, violent attacks did not break working-class solidarity. In 1887, as in later major labor-management confrontations, expelling organizers did not end resistance. In every case, only prolonged employer lockouts, coupled with the threat of brutality and official support for it, forced Tampa's nonviolent cigar workers to accept management's terms. Even then, workers returned to their jobs as a unified group.

Outside the scope of this book is an explanation of what lay behind such solidarity. What working-class culture tied immigrants from Italy, Cuba, and other places into a cohesive unit? Ingalls does give one intriguing example: the lector who read to cigar workers in the factories. The selections read were often radical, and cigar workers staunchly protected this

institution until it finally disappeared with the rationalization of labor-management relations in the 1930s.

Only with that rationalization, Ingalls argues, did violence cease to be a weapon for enforcing the status quo. At that time, the recognition of unions gave employers a new way to discipline workers. Therefore, newspapers that had previously advocated violence condemned the vigilante murder of political dissident Joseph Shoemaker in 1936. Even in that case, however, none of the vigilantes were convicted, reflecting the continuity of old standards even as new ones emerged.

MARC S. MILLER

Massachusetts Institute of Technology

PAUL K. CONKIN. *The Southern Agrarians*. Knoxville: University of Tennessee Press. 1988. Pp. xi, 196. Cloth \$24.95, paper \$12.95.

This excellent study of the Southern Agrarians by Paul K. Conkin rescues them from the murky realm of myth and symbol. Combining group biography and intellectual history, Conkin examines a varied group of artists "as moral and political activists." His purpose, he says, is to force "people to confront their own identity" (p. x). His book is a clear and comprehensive account of the life, work, and death of the Southern Agrarian movement.

The Southern Agrarians emerged from intense discussions among about twenty people in Nashville, Tennessee, from 1915 through 1937. The movement began among a dozen young poets who, from 1922 through 1925, published a small monthly journal, *The Fugitive*. Most of the men were students or faculty at Vanderbilt University. Prominent among them were John Crowe Ransom, Donald Davidson, Sidney M. Hirsch, Allen Tate, and Robert Penn Warren. After 1930 four members of that group were joined by others and became known as the Agrarians. Whereas the Fugitives were interested primarily in literary subjects, the Agrarians assumed broader philosophical and political commitments. All natives of the South and led by Ransom, Davidson, and Tate, they began to defend their homeland against its critics. No shallow apologists, they were deep thinkers who attempted to incorporate a love for southern manners, politics, and oratory into an enduring philosophical system.

Joined by Andrew Lytle, Frank Owsley, John Donald Wade, and others, they put together a book of twelve essays that expressed their manifesto. *I'll Take My Stand*, published in 1930, was loose, diverse, and subject to almost infinite interpretation. The essayists shared a view of southern history in which family, place, leisure, and religion were dominant themes. In the wake of the book, an Agrarian movement emerged that survived only from 1933 through 1937. In essays, symposia, and creative writing, the Agrarians sought to reform the South. Since they had no means except their writings to bring change or to stem the march of time, "Agrarianism became the second lost cause"

(p. 127). The literary men retreated into their art and Owsley into his innovative histories. Their own fates and personalities were as diverse as the region about which they wrote.

Conkin makes a convincing argument that the Agrarians should be taken seriously. They were social critics and reformers, he says, who "led the last significant campaign in behalf of property and the humane concerns so well expressed in the ancient right of property" (p. 171). Although changes in the South after 1937 outdated their remedies, "they remind us of what people lost in the transition from a proprietary to a collective economy" (p. 178). Conkin demonstrates with uncommon skill that ephemeral truths of literature, history, and life spring from the realities of individual personalities, social interaction, patterns of human thought, time, and circumstance. By applying the craft of the historian to the analysis of one point where art and history intersected, he has opened large new possibilities for understanding the Southern Agrarians.

E. STANLY GODBOLD, JR.
Mississippi State University

DONALD R. WALKER. *Penology for Profit: A History of the Texas Prison System, 1867-1912*. (Texas A&M Southwestern Studies, number 7.) College Station: Texas A&M University Press. 1988. Pp. 216. \$24.50.

Donald R. Walker chronicles the development of Texas prisons from the adoption of the infamous lease system in 1871, through a transitional "state as lessor" system that started in 1883, to 1912, when the state ended leasing completely by bringing convicts under its own control at institutions and associated state farms. Insofar as Walker presents a thesis, he argues that the Texas prison system "did not evolve as a result of any clearly conceived and well-executed master plan" but rather "from the plodding and often haphazard efforts of state officials to provide for a growing inmate population at a time when there was little money . . . and limited public support for innovation in prison policy" (p. 4). Although he is aware of a recent federal court decision exposing persistent, systematic corruption within Texas prisons, Walker concludes that, by the mid-twentieth century, these prisons had joined "the first rank of such institutions nationwide" (p. 198).

Lack of sophistication characterizes this book, literally from first page to last. Although we now have a rich literature on the development of prisons in general, and in the South in particular, Walker seems unaware of most of this work and the historiographical issues that it has raised. He relies mainly on official reports, sometimes taking administrators' declarations of intent at face value, and he draws information from a criminal justice textbook rather than from recent secondary works on the prisons of Texas. In contrast to Edward L. Ayers's excellent *Vengeance and Justice: Crime and Punishment in the Nineteenth-Century American South*

(1984), this book's organization is merely chronological. When themes do surface, they are submerged by details, many barely relevant to the subject. The tables are poorly conceptualized and poorly presented, and, in at least one instance, their statistics conflict with those of published reports.

For the most part, Walker focuses on male prisoners, ignoring important differences in the treatment of female convicts. He also pays too little attention to race. Walker rejects two of the major conclusions of other historical analyses of southern prisons: first, the argument that, just after the Civil War, states of the region passed "pig laws" to bring newly freed blacks under control of the criminal justice system, and, second, the contention that, in the postbellum period, southern prisons became, in effect, replacements for slavery. Yet he does not give sufficient evidence to support his rejection of these theses, and, although he does, in passing, note a few ways in which black convicts were treated more harshly than whites, he slights the significance of such differences. The only point at which he gives racial issues the attention they deserve is in his discussion of the resistance of free blacks to penal policies—the most interesting passage in the book.

Walker is more attuned to the close and often self-serving links between prison administrators and politicians. He also carefully traces the growth of public opposition to leasing on the ground that prisoners were taking jobs from free labor. But self-interest never becomes an analytical theme. For the venality that characterized leasing, Walker tends to blame either economic factors (state poverty) or personal weaknesses (individuals' moral failings).

Despite more than one hundred years of what Walker characterizes as reforms, Texas prisons, in the view of many observers, remain among the worst in the nation. An approach that gives more weight to social and organizational factors is required to explain their long history of inhumanity.

NICOLE HAHN RAFTER
Northeastern University

J'NELL L. PATE. *Livestock Legacy: The Fort Worth Stockyards, 1887-1987*. (The Centennial Series of the Association of Former Students, Texas A&M University, number 27.) College Station: Texas A&M University Press. 1988. Pp. xix, 332. \$29.95.

For this history of the stockyards at Fort Worth, Texas, J'neil L. Pate consulted company records, manuscript collections, newspapers in the Fort Worth area, government documents, and the relevant secondary accounts. Especially noteworthy is her effective use of interviews with former employees and other individuals. The result is a volume that complements or supplements recent books by Louise C. Wade, Mary Yeager, and Jimmy M. Skaggs and that will be useful to historians of agriculture, business, and the West.

Fort Worth was a cow town in the days of the

Chisholm Trail. When the first railroad arrived in 1876, local entrepreneurs began to consider the feasibility of developing a terminal market and meat-processing enterprises. They did so in the next decade, only to encounter problems that resulted in the acquisition of the properties by Boston investors in 1893. The new owners fared little better, although in 1896 they instituted a fat stock show that survives. What was needed, apparently, was the arrival in Fort Worth of one or more of the Big Four in meat packing. Success came in 1902 when Armour and Swift put up major meat-processing plants. They each became one-third owners of the new Fort Worth Stock Yards Company, with J. Ogden Armour as the first president. Soon the packers owned, in addition, a stockyards newspaper, a beltline railroad, a bank, a cattle loan company, and a townsite firm. Business grew satisfactorily during the first decades of the twentieth century, and the Fort Worth market came to be comparable to those in Kansas City, East St. Louis, and South St. Paul. Some thought that Fort Worth might surpass Chicago as the top livestock market in the country. It failed to reach that goal, partly because Texans could not be induced to raise hogs in the required numbers.

Pate is remarkably comprehensive, perhaps excessively so, in describing the varied aspects of stockyards operations. Included are such matters as the work of the commission firms, the character and problems of the work force, and relations with the city of Fort Worth. The author also discusses governmental regulation of livestock markets, explores the question of oligopoly in meat packing, and explains how control of the yards was changed when the courts forced the packers to give up ownership in the 1920s.

The great terminal markets, including the one at Fort Worth, began to lose their premier place in the 1920s and 1930s. The trend continued in the post-World War II era, highlighted by the closing of Chicago's Union Stock Yards in 1970. Everywhere the causes were the same: the increasing use of trucks to haul livestock, the appearance of local markets that were regulated less stringently, direct marketing, and the shift of cattle feeding from the traditional farm feedlots. At Fort Worth, the closing of the packing plants of Armour and Swift in 1962 and 1971, respectively, marked the end of the operations of the major stockyards there. In the 1980s, some marketing remains, but the area of the old yards is best known for Billy Bob's Texas, a nightclub billed as the world's largest. Ironically, it caters to urban cowboys and features mechanical bulls.

ROY V. SCOTT
Mississippi State University

ROBERT M. UTLEY. *Cavalier in Buckskin: George Armstrong Custer and the Western Military Frontier*. (Oklahoma Western Biographies, number 1.) Norman: University of Oklahoma Press. 1988. Pp. xvii, 226. \$19.95.

In response to the question whether we need another book on George Armstrong Custer, Robert M. Utley has answered in the affirmative with his well-written, richly illustrated critical biography of the legendary military figure. It is entirely fitting that the author, with years of research in the fields of western military and Indian affairs, should share his conclusions on the manner of man that Custer was.

Americans celebrated the life of Custer in the years after the Battle of Little Bighorn until Custer's story reached heroic proportions and the dashing Indian fighter took an honorable place in the nation's popular culture beside such greats as Daniel Boone and Buffalo Bill. Starting in 1934 and reaching a peak in the 1960s and 1970s, a negative image of the soldier emerged in the form of a glory-seeking, egotistical, and even maniacal persona. Utley's portrayal of Custer is balanced but positive; he richly illustrates a man with character weaknesses but one who emerged from the Civil War with an unblemished record of achievement and who gained the greatness he desired in death on the western battlefield in southern Montana.

Custer was a slovenly cadet who finished last in his West Point graduating class, but, during the Civil War, he developed from a raw second lieutenant at the war's beginning to a boy general. He reached martial maturity at Gettysburg, honed his skills in the Valley of Virginia campaigns, and topped off his experiences with brilliance in the battles around Richmond, culminating with his acceptance of Confederate General Robert E. Lee's surrender flag outside of Appomattox.

In the post-Civil War period, Generals William T. Sherman and Philip Sheridan selected a handful of top subordinates who they felt would serve the nation as aggressive Indian fighters. Custer was one of the most respected of these men, although the record he achieved did not always reflect the hopes that Sherman and Sheridan had for him. In Texas immediately after the war, he mishandled the training of recruits. In campaigns on the southern plains against the Cheyenne Indians, he did not distinguish himself, and eventually his actions led to court martial for being absent without leave and a guilty sentence that resulted in a year's suspension from active duty. He later redeemed himself with victories over the Cheyenne, well-conducted expeditions escorting railroad survey teams, and an exploration of the Black Hills. From these experiences Custer emerged a "shrewd and skilled Indian fighter, mighty hunter, and master plainsman . . . a cavalier in buckskin" (p. 78).

Utley's greatest contribution is perhaps that of uncovering the private Custer. His complex personality allowed his associates to either love or hate him. There seemed no middle ground. He probably was sexually promiscuous before and after his marriage. He also practiced nepotism to a ridiculous degree, and, although he appeared to many as a man of honor and integrity, he likely was guilty of unethical, dishonest, and even unlawful acts that defrauded the government. Custer was a man of contradictions, but, envel-

oped in the mystery that surrounded the tragic Little Bighorn, he became "a figure of towering significance" (p. 211).

Utley's synthesis of hundreds of books, articles, paintings, and photographs and thousands of pages of primary sources is impressive. To condense this massive amount of material into a two-hundred-page biography warrants extensive praise. That some episodes and details of Custer's life would be omitted is understandable. One might have hoped, however, for Utley's appraisal of Custer's treatment of prisoners in the Valley of Virginia during the Civil War and a fuller coverage of General Alfred Terry's written orders to Custer prior to the "Last Stand." Although Utley addresses the often-debated and controversial question of whether Custer disobeyed his orders, the discussion would have been more fairly presented had Utley spelled out the significant first part of the orders. These trivial matters aside, this first book in the Oklahoma Western Biographies Series is an evocative book that expands the historiography on Custer beyond the extremes of the past to a more balanced and appropriate place in American frontier history and popular culture.

JOHN W. BAILEY
Carthage College

SANDRA SIZER FRANKIEL. *California's Spiritual Frontiers: Religious Alternatives in Anglo-Protestantism, 1850-1910*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xv, 182. \$30.00.

In recent years, scholars such as R. Laurence Moore and Robert Ellwood have challenged the dominant "consensus" interpretation of the role of religion in American life. In place of the Protestant mainline, or civil, religion or a common faith, these scholars have insisted that a radical pluralism has existed in American religion that gives evidence of the widely diverse and often contradictory factions in American culture. Unfortunately, this scholarship has been as vague and amorphous as the interpretation it criticizes.

Sandra Sizer Frankiel's book is an important contribution to this emerging body of literature, one that is far more detailed and far more specific than any of the previous studies. By limiting herself to a specific population with an identifiable ethno-religious heritage (white, Anglo-Saxon Protestants [WASPs]) in a specific place (California) during a period of rapid social and economic change (1850-1910), Frankiel has yielded an impressive account of the diversity of a seemingly homogeneous people. Her treatment is more analytical than narrative, and the result is a book rich in insights.

Frankiel concentrates on theological liberalism, spiritualism, Adventism, the "new mysticism," and the Holiness movement. Each of these represented a challenge to mainline Protestant orthodoxy and authority, yet their adherents were overwhelmingly WASPs. Frankiel suggests a relationship between these emerg-

ing movements and social tension within the Anglo-Protestant community. The suggestions are worth considering, but the vital analytical power of the book is in Frankiel's treatment of the differing cosmologies represented by each of these movements. Her close, informed, and intelligent reading of the relevant documents provides an excellent foundation for her speculations about the social composition of these groups. The chapter "Mainstream Churches and the New Mysticism" is particularly useful. Frankiel demonstrates the impact that Theosophy, New Thought, and Christian Science had on established denominations. Her analysis is a healthy corrective to the treatment of religious currents as mutually exclusive in American culture.

One can find a few things to quibble about, if one wishes. Lumping Theosophy, New Thought, and Christian Science together may be a bit confusing to the general reader but will make perfect sense to specialists in American religious history. Theological liberalism, spiritualism, Adventism, and the Holiness movement may be more deeply rooted in the mainline Protestant tradition than Frankiel indicates. But such quibbles do not detract from the power of Frankiel's argument. Indeed, these considerations ultimately reinforce her interpretation.

That interpretation, briefly stated, is that American Protestant culture in California lost its integrative power during these years for significant portions of the population. Relative isolation, weak family connections, and ephemeral communities created a search for new cosmologies. This interpretation is well worth considering, even if one ultimately disagrees. I published a book ten years ago that emphasized the conservative continuity within the Protestant community in California. As a result of reading Frankiel's latest work, I am rethinking my own assumptions. Even if I ultimately reaffirm my interpretation, I nevertheless welcome Frankiel's book as an important contribution.

GREGORY HOLMES SINGLETON
Northeastern Illinois University

ROBERT E. FICKEN and CHARLES P. LEWARNE. *Washington: A Centennial History*. Seattle: University of Washington Press, with the assistance of the 1989 Washington Centennial Commission. 1988. Pp. xxi, 216. \$17.50.

A funny thing happened on the way to Washington's one-hundredth anniversary: two competent, hard-working authors found themselves with less than one year to write a new history of Washington State under contract to a commission arranging the 1989 centennial celebration of statehood. The result is predictable: an uneven, hasty survey that is drawn mainly from secondary works and is strongest in topics the authors have previously studied. Robert E. Ficken and Charles P. LeWarne, who is the author of a state history for secondary schools, nonetheless cram in original research in the papers of Senators Warren G. Magnuson

and Henry M. Jackson and recently declassified documents on the Hanford Works, maker of the atomic bomb.

Four key developments, the authors conclude, shaped the state's history: Captain Robert Gray's discovery of the Columbia River "laid the foundation for subsequent American claims to the Northwest coast"; the demand for lumber during the California gold rush of 1849 "commenced a century-long dominance of Washington industry by loggers and lumbermen"; the Northern Pacific Railroad's transcontinental line ended the Northwest's geographic isolation; and the Japanese attack on Pearl Harbor resulted in a "mammoth expansion of production" from war industries relying on hydroelectric power abundant in Washington (p. 184).

Most of this short survey consists of a chronological listing of events the authors regard as significant, typical, or seminal. There is scant room for characterization or interpretation, but Ficken and LeWarne advance some interesting insights. They suggest that rather than being unique, as Washingtonians have routinely claimed, the state merely reflects its early isolation (p. 185); that environment "achieved equivalent importance" with lumbering, "a moment of great symbolic importance in Washington history," with creation of Olympic National Park (p. 125); and that eventually "Seattle, and to a large extent the state, became dependent upon the success" of the Boeing Company (p. 145).

One of the dilemmas of Washington historians is that "Western and Eastern Washington, separated by the snowy Cascades, have always been regarded as two distinct provinces" (p. 58), a political welding of opposites. Ficken and LeWarne know western Washington better than eastern Washington, and their discussion of the fiercely parochial eastern side is, unfortunately, marred by factual and interpretive errors.

The authors offer a list of sources and suggested reading, which they describe as "more a guide to additional reading than a comprehensive list of sources" (p. xi). It is dismaying to discover on the list Rose Boening on irrigation and W. Hudson Kensel on lumbering, Robert Nesbit and Charles Gates on eastern Washington agriculture, and George Sundborg on Grand Coulee Dam, all useful studies in their day but now superseded by more recent works.

The authors, aiming their book at the general reader, divided responsibility for chapters and then traded drafts. They achieve a single voice, and the writing is not without grace. In an introduction, ten topical chapters, and an afterword, Ficken and LeWarne carry the state's history from discovery nearly to the present. Washington needs an authentic account of its history. Given adequate time, this capable pair could write it.

JOHN FAHEY
Eastern Washington University

ARI HOOGENBOOM. *The Presidency of Rutherford B. Hayes*. (American Presidency Series.) Lawrence: University Press of Kansas. 1988. Pp. x, 278. \$25.00.

Rutherford B. Hayes is remembered, if at all, for his association with the disputed election of 1876. Ari Hoogenboom begins his book with a thorough but concise account of the twists and turns involved in getting Hayes into the White House, dismissing the "Compromise of 1877" and stressing the Ohio politician's refusal to make concessions to win office. Among many other intelligent observations, Hoogenboom notes that "contemporaries and historians have usually focused on the politicking that led to winning the prize in 1876/77 and again in 1880, rather than on how the prize was used" (p. 193). How Hayes used his prize, the presidency, is the primary focus of this volume.

A moderate and a pragmatist, President Hayes showed caution in economic and foreign affairs. The Panic of 1873 signaled the beginning of a long depression that bottomed out during Hayes's term, and the nationwide railroad strike it triggered in 1877 tested the president's astuteness. His measured responses, which involved delaying the use of troops to avoid confrontations between the U.S. government and the people, appear to have benefited all parties involved. A monetary conservative, he opposed expanding the circulation of greenbacks and silver coinage, and his stubbornness helped the United States maintain the gold standard. A similar conservatism was evident in the Hayes administration's handling of international relations, except when Ferdinand de Lessups announced his impractical decision to dig a sea-level canal through Panama. Hoogenboom argues that Hayes's nationalistic response foreshadowed Theodore Roosevelt's behavior a quarter of a century later when Roosevelt announced his corollary to the Monroe Doctrine.

The Hayes administration tried to deal fairly and sympathetically with both American Indians and southern blacks. Although the U.S. army pursued Chief Joseph of the Nez Percé and his followers almost into Canada, the president and Secretary of the Interior Karl Schurz reduced fraud in the Indian Affairs Office and continued the "peace policy" begun under Ulysses S. Grant. A series of confrontations with Democratic majorities in the House of Representatives and, after 1879, in the Senate hobbled Hayes's southern initiatives. Despite his repeated vetoes of Democratic attempts to undermine federal voting rights activities, he could not prevent a rapid decline in Republican party influence in the South and, with it, black political power.

An honest and highly principled man, Hayes attempted to move the United States toward acceptance of a professional, nonpartisan civil service. No historian is better qualified to describe the character and operation of the Gilded Age spoils system than Hoogenboom. Chapter 5, entitled "The Federal Bureaucracy," is particularly enlightening. It details the size and scope

of the federal government in the 1870s, describing the numbers and types of employees in each cabinet department and how they functioned. This chapter serves as an ideal background for Hoogenboom's incisive discussion of civil service reform. The patronage system and its associated political bosses had become so entrenched and institutionalized by 1877 that even Hayes's willingness to ignore or alienate powerful Republican leaders failed to gain him much headway in combating abuses of the system. It is arguable, however, that without the attention his public statements and action attracted, the installation of a merit system would have been delayed long past the Pendleton Act of 1883.

This volume is another excellent addition to the American Presidency Series published by the University Press of Kansas. It is readable, summarizes complex topics succinctly and clearly, and provides comprehensive endnotes and bibliographical information. Best of all, it illuminates some of the more neglected aspects of an already neglected period, providing a history of the federal government as a whole as well as the presidency of Rutherford B. Hayes.

JOHN M. DOBSON
Iowa State University

STEVEN P. ERIE. *Rainbow's End: Irish-Americans and the Dilemmas of Urban Machine Politics, 1840-1985*. (California Series on Social Choice and Political Economy, number 15.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xiii, 345. \$27.50.

This book contains a stimulating, far-ranging, and provocative examination of the evolution of machine politics in America over the past one hundred fifty years. Steven P. Erie examines eight American cities with a history of Irish-run urban political machines—New York, Philadelphia, Chicago, Boston, San Francisco, Pittsburgh, Jersey City, and Albany—and presents an impressive argument to explain the development of those machines. He asserts that all machines exhibited certain common tendencies, notably, that power was centralized in the hands of a single party boss, that the machine's power was citywide, that these machines exhibited staying power over time, and that they provided important material benefits such as patronage jobs and, later, welfare services. Erie finds variations both among these cities and within specific cities over time. He determines that there were four chronological stages of machine development: 1840-96, a time of conservative fiscal, patronage, and labor policies mandated by the strength of Republican opposition to machines at the local and state levels; 1896-1928, a phase of growth that mobilized "old" but not "new" immigrants; 1928-50, the period of the New Deal, which represented either an opportunity for machine building through the capture of federal patronage or a period of destabilization for those machines unable to do so; and 1950-85, the final stage of

machine development, characterized by low taxes, homeowner services, and federal welfare programs. In Erie's analysis, two cities, Boston and Philadelphia, were never able to piece together mature urban political machines.

In presenting his conclusions, Erie challenges a variety of earlier interpretations and focuses on two in particular. He argues that "it is time to lay the rainbow theory . . . to rest" (p. 6), and he disputes what he calls the "mass" theory of machine politics articulated by scholars such as Edward Banfield, James Q. Wilson, and Daniel Patrick Moynihan, who have suggested that machines were a reflection of an ethnic group's values and social structure. He also takes issue with the "elite" theory proposed by Martin Shefter, who has speculated that urban bosses built successful political machines by creating a winning combination of factors, including the necessary votes, a system of reward and discipline for the party's precinct and ward captains, control of public officials, and adequate party financing. For Erie, the explanation of how the urban political machine evolved rests on the "supply side" of local party organizations—their resources, particularly patronage jobs—and on the alliances of local bosses with state and national party leaders. He argues that those intricate intergovernmental alliances created resource monopolies that were essential for consolidating local machines. Erie notes that urban bosses had to manage voter demands and so developed methods that, over time, limited voter claims on the machine's limited resources. He insists further that the machines were not representative of the collective will of the immigrant working class, were imperfect avenues for group social mobility, and did little to assimilate new ethnic groups.

Erie presents a forceful and often convincing argument that achieves a synthesis of what has been said on urban political machines, and he offers a theory that integrates several dimensions of urban political experiences over time. He argues persuasively that historians need to emphasize intergovernmental relations and to understand the growth of urban political machines within the context of the state and national events that affected them. At the same time, however, Erie has produced a study that is largely politics with the people left out. Although we learn a great deal about the machines, the bosses, and political theory in general, Erie treats ethnic groups as voting blocs rather than as collections of individuals, factions, or competing interest groups linked by background and a sense of tradition, which is itself shaped over time and which shaped any group's view of the machine. By focusing on politics and economics, Erie diminishes the important social, religious, and psychological dimensions that affected machine development in often intangible ways, particularly in cities such as Boston in which mature machines did not develop.

Despite these weaknesses, Erie's study deserves a wide readership. It documents the growing sophistication of studies of Irish Americans, demonstrates an

impressive use of primary sources, and points to the need for greater synthesis.

BRIAN C. MITCHELL

National Endowment for the Humanities

JOHN F. REYNOLDS. *Testing Democracy: Electoral Behavior and Progressive Reform in New Jersey, 1880–1920*. Chapel Hill: University of North Carolina Press. 1988. Pp. xvi, 245. \$39.95.

Voter participation in national elections has been declining for a hundred years. New Jersey's voter turnout, for example, peaked at 95 percent in the presidential election of 1880. By 1916 it had dropped below 70 percent, and, in the 1918 off-year election, it plummeted to 43 percent. Recent scholarship about voter decline is focused on the decades before and after 1900 and suggests that the reforms of the Progressive era were a contributing factor. John F. Reynolds agrees and, in his "microanalysis" of the Garden State, goes further to examine other aspects in the voter decline.

The starting point of the decline was the introduction of the Australian secret ballot in 1890. State party leaders supported the reform, which increased their power at the expense of the local party organization. The secret ballot did not substantially decrease voter participation right away, but local clubs, torchlight parades, and street pageants began to disappear, reflecting fewer resources to mobilize voters at the grass roots.

Next, William Jennings Bryan's nomination in 1896 split New Jersey Democrats. Affluent party members were among those who stayed home or voted against the ticket, reducing local funding and voter mobilization. The election put the state into the Republican camp nationally for a generation. After 1900 Democrats who backed Theodore Roosevelt were urged to support their party at the local level. Such ticket splitting further encouraged independent voting at the expense of party loyalty.

Meanwhile, the arrival of the new middle-class, progressive attitude toward government further weakened political parties. State control of party primaries and civil service reforms increased the role of government at the expense of the party organization. Campaign tactics also changed as candidates appealed directly to voters through whistle-stop tours and the media. Issues became more important. With more ticket splitting and independent voting, candidates increasingly wooed nontraditional supporters in lieu of maximizing the partisan vote. Thus, Woodrow Wilson's gubernatorial victory in 1910 reflected a substantial crossover, independent vote on top of a partisan base.

Increasingly, too, voting was seen by progressives as a civic responsibility rather than simply a partisan act. Campaigns sought to educate voters for informed choices, and critics worried about the ignorant, supposedly irresponsible black or immigrant voter. Reformers preferred a smaller educated citizenry over massive turnouts sponsored by the urban political machines.

Jersey City's notorious mayor Frank Hague ridiculed the progressive efforts to develop a responsible electorate, claiming that, in any election, "a full fifty percent of the voters have got to be coaxed or dragged to the polls" (p. 124).

The climax of the progressive reforms came in 1911 following Wilson's inauguration. Legislation was passed confining voter registration to just four days prior to the elections, and registration renewal was required any time a voter missed an election. Mailing and police surveillance were authorized to validate a prospective voter's residence. A new lengthy and complicated ballot prohibited voting for a straight ticket.

Progressives celebrated the resulting decline in voting to 54.2 percent of the electorate in 1911, claiming that fraudulent and floating voters had been eliminated. Further study showed, however, that black and immigrant voters also had been removed from the books. In effect, "the legacy of Progressive reform was a devitalized electoral system" (p. 159).

Reynolds substantiates his conclusions by using a variety of statistical procedures, particularly regression analysis and estimates of voter and party turnout, factor analysis, Pearson correlations, and multiple classification analysis. Some seventy tables and graphs enhance the text.

My only major concern about this study relates to the urban, ethnic character of New Jersey during these years. By 1880 a majority of the people lived in urban areas, and the proportion grew over the next forty years. In addition, first- and second-generation ethnic residents increased from one-third to three-fifths of the population. Reynolds suggests that this demographic profile anticipated twentieth-century northeastern America, yet the state became predominantly Republican in 1896 and stayed that way. Granted, the suburbs grew rapidly also. Still, some additional explanation about the urban, ethnic vote is needed. Were some cities predominantly Republican? Which ethnic groups deserted the Democrats? That the progressive reforms passed suggests a larger middle-class, independent, and progressive Republican or Democratic vote than the demographics suggest, especially in a state that produced Hague.

That concern aside, Reynolds adds to our understanding of Gilded Age and Progressive-era electoral politics and reform, their ambiguities and complexities, and their consequences for twentieth-century America.

JAMES B. CROOKS

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EDWIN GABLER. *The American Telegrapher: A Social History, 1860–1900*. (Class and Culture.) New Brunswick, N.J.: Rutgers University Press. 1988. Pp. viii, 264. Cloth \$45.00, paper \$20.00.

During the summer of 1883, the telegraph operators, clerks, and linemen employed by Western Union called a nationwide strike against the communications giant.

Knights of Labor assemblies throughout the country rallied to support the telegraphers, and dozens of newspapers reported favorably on the dignified conduct of the strike. But Western Union proved a resourceful foe, putting supervisory personnel and thousands of strikebreakers to work on the wires. As the unionists' hopes for a quick victory were dashed, desperate linemen began cutting telegraph wires, but to no avail. The strike ended amid general bitterness, and harsh words of recrimination passed between telegraphers and the national leadership of the Knights of Labor, who condemned strike leaders for poor planning. The cause of unionism in the telegraph industry all but died, and telegraphers were left unprotected against their employers' unrelenting efforts to cut labor costs.

Although the telegraphers' strike was the first nationwide strike backed by the Knights of Labor, it has received no attention from historians for a century. Now Edwin Gabler has reconstructed the history of telegraphers' unionism. In his book, Gabler tells the story of the 1883 strike in a lively opening chapter and then explores its social significance in four wide-ranging chapters.

Gabler explores a paradox: with the backing of the labor movement, telegraph operators organized a bitter and highly effective strike against one of the nation's most powerful corporations, despite the fact that the strikers were relatively well-paid, middle-class, white-collar workers who had little in common with the vast majority of the workers who constituted the labor movement of the Gilded Age. Gabler describes the "Knights of the Key" as dignified, well-dressed, almost dandified young men who spent their every penny on fine clothes and "fast" living. From his analysis of the trade's publications, he concludes that telegraphers shared with other middle-class Americans a concern for self-improvement and social ascent; their models were telegraphers who made good, such as Andrew Carnegie and Thomas Edison.

Gabler devotes much attention to the female telegraphers who operated hundreds of small town stations, performed clerical work in big-city bureaus, and made up a significant proportion of second-class telegraphers as well. Despite the fact that employers relentlessly relegated women to low-paying jobs and despite the fact that most males in the industry agreed that women could not handle the high pressure of first-class positions, the women telegraphers displayed great solidarity during the strike, provided significant leadership in keeping the men strong when spirits flagged, and succeeded in gaining widespread acceptance for their demand for equal pay for equal work.

While Gabler's book offers a fascinating glimpse into a white-collar world that labor historians usually ignore, it would have been strengthened by a more rigorous analytic approach. Western Union's ruthless wage cutting, despite its near-monopoly position, is explained, but Gabler's economic analysis bypasses the market for telegraphy. The reader never learns who the customers for

telegraphy were or whether the volume of telegraphic work was increasing or slackening in the late nineteenth century. Without a rigorous analysis of the market, any explanation of declining wage rates—a central issue in the strike—must be suspect.

Second, Gabler's central argument—that telegraphers were part of a new middle class created by the growth of corporate capitalism—would have been stronger if he had systematically compared telegraphers' social orientation with that of workers in other working-class and middle-class occupations. Gabler's demographic analysis indicates that most telegraphers were children of working-class parents, especially Irish immigrant skilled and unskilled workers, and Gabler argues that working the telegraphic key was a step up socially as well as economically, a journey into a different cultural world. But Gabler's description of the boisterous, uncouth working-class culture telegraphers left behind does not ring true. Many skilled workers dressed in suits and hats to go to work and fervently valued their respectability and reputation. If telegraphers were truly distinct from such journeymen, Gabler has not demonstrated the point.

Finally, this book raises an important theoretical issue. In a recent essay in *Radical History Review*, Alan Dawley located class consciousness of the late nineteenth century in workplace struggles. According to that definition, Gabler's middle-class telegraphers possessed working-class consciousness. Clearly, there is work to be done in clarifying the relationship between social status and social action.

DAVID BENSMAN
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New Brunswick

RONALD A. SMITH. *Sports and Freedom: The Rise of Big-Time College Athletics*. (Sports and History.) New York: Oxford University Press. 1988. Pp. xiv, 290. \$29.95.

Ronald A. Smith's study examines the rise of intercollegiate athletics from the start of the second half of the nineteenth-century until the creation of the National Collegiate Athletic Association in 1906. The work focuses on three themes: the rapid adoption of a professional sport model disguised in the name of amateurism; the shift from student to institutional control; and the preeminent role that Harvard and Yale played in shaping the tone and direction of intercollegiate athletics.

Smith effectively exposes the myth that a lengthy period of amateur sport ever existed on the collegiate level. The early emphasis placed on winning as a means of enhancing institutional prestige led quickly to the commercialization of intercollegiate athletics, the hiring of professional coaches, the adoption of rational training techniques, and a host of recruitment violations. Smith's work is most innovative and insightful when he examines the organizational development of intercollegiate athletics. He provides an informative

discussion of the continuing friction between the students' wishes for freedom and autonomy and the faculty's desire for control and authority. While by the 1880s the faculty had wrested control, Smith analyzes why their efforts to establish interinstitutional regulations failed to reform college sports. Not until the football crisis of the early 1900s raised the specter of the abolition of that sport did colleges move to create a national organization to govern and control intercollegiate athletics.

The work is less successful in explicating the connections between sport and societal changes. Smith takes note of the major societal movements of the period, but he does not sufficiently explore how they shaped both the legitimization and the development of intercollegiate athletics. The study also does not scrutinize in detail how the significant changes in the nature and structure of higher education affected the direction taken by college sport. Smith informs us that, similar to other arenas of higher education, governing boards, comprised largely of alumni, came increasingly to control college athletics and usurp some of the former prerogatives and power of both faculty and university presidents. He should, however, have investigated in far greater depth how this process unfolded, and his study would have benefited from an examination of the social backgrounds of those individuals who served on the athletic councils of at least a handful of representative institutions.

Smith clearly demonstrates the prominent role Harvard and Yale played in the development of intercollegiate athletics, but greater attention should have been given to the rise and problems of sport at other institutions. All too often the discussion of sport elsewhere appears to be included to amplify the centrality of America's two leading universities. The work also does not adequately examine how different conditions at the newly created state universities, especially in the Midwest, altered the tone and meaning of intercollegiate sport.

This study scores points for its treatment of several themes related to the rise of big-time college athletics, but far too many questions remain unanswered to proclaim victory.

MELVIN L. ADELMAN
Ohio State University

WAYNE PATTERSON. *The Korean Frontier in America: Immigration to Hawaii, 1896-1910*. Honolulu: University of Hawaii Press. 1988. Pp. xii, 274. \$30.00.

In January 1903, the *Gaelic* arrived in Honolulu carrying 102 Korean laborers bound for the Hawaiian sugar plantations. They were the first of more than 7000 who would come to the islands in the next two years, and, although this influx was neither as massive nor as long-term as that from Japan, Wayne Patterson believes that it significantly shaped twentieth-century Asian-American relations.

This previously untold story has its compelling elements. The Hawaiian Sugar Planters Association longed to import Koreans to dilute Japanese domination of the plantation work force, thereby depressing wages and reducing the number of strikes. Yet underwriting Korean migration posed problems: the procurement of contract labor, following Hawaii's annexation in 1898, violated U.S. labor codes. To circumvent those, the planters relied on the duplicitous Horace Allen, U.S. minister to Korea. Through his close association with Emperor Kojong, Allen arranged for American businessman David W. Deshler to serve as the official recruiter, and together they obscured the planters' involvement.

The Koreans were also active partners in the migration process. Emperor Kojong's support was predicated on his hope that one result would be closer Korean-U.S. relations, effectively neutralizing Japanese ambitions for the peninsula. As for the Koreans who actually sailed east, Patterson surmises that migration was an escape from the political turbulence and economic dislocation that marked late Yi Korea.

In the end, these various desires conflicted with Japan's imperialism and its effort to protect its citizens in the United States. When Japan imposed a protectorate over Korea in 1905, it prohibited further migration to Hawaii, hoping to raise wages in the islands and slow the Japanese exodus from them to higher-paying jobs on the West Coast, consequently easing anti-Japanese sentiment in America. These actions dovetailed with President Theodore Roosevelt's willingness to facilitate Japanese expansion: international comity took precedence to an independent Korea.

Fair enough. But there are problems with this account. The quality of Patterson's analysis, for instance, is uneven; his grasp of things Korean is more solid than is his understanding either of Hawaii's complex politics or of Roosevelt's diplomatic initiatives. The quality of his writing, moreover, is uniformly poor. This book is choked with grammatical errors; the stilted narrative cripples its argumentation.

And then there is the appendix, a compilation of oral histories of Korean Americans in Hawaii. Here Patterson observes that immigrants are usually "not attuned to the historical importance of the movement in which they are participating" (p. 183), a patronizing observation that these stories eloquently refute. The Koreans knew why they migrated, knew, too, why they struggled to maintain their cultural traditions in an alien land. Had Patterson integrated these stories into the text, his pedestrian study would have come alive.

CHAR MILLER
Trinity University
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ROBERT A. ROSENSTONE. *Mirror in the Shrine: American Encounters with Meiji Japan*. Cambridge: Harvard University Press. 1988. Pp. xiv, 315. \$25.00.

Robert A. Rosenstone has collaborated in making two of his previous works into movies and is currently writing a book on the challenges of portraying history in film. This volume is not about cinema; it is cinema, complete with verbal fade-ins and fade-outs, many wonderful moments, and many inflated ones.

Rosenstone's subject lends itself to technicolor. He focuses on the infatuations of three American visitors with the grace and harmony of nineteenth-century Japan. We follow evangelical William Elliot Griffis from his first look at Mount Fuji, to his solitary sojourn in the feudal interior, to his retreat in cultural and sexual confusion to cosmopolitan Tokyo under the guardianship of his spinster sister. We also follow Edward Morse, shell collector, as he develops an interest and admiration for Japanese folk arts, classifies pottery, and finally takes lessons in the ritual tea ceremony and recondite *No* singing. And we follow Lafcadio Hearn, misfit, romantic, and author, whose odyssey leads him to go completely native to marriage and citizenship in Japan.

Rosenstone's interwoven account of these men, all of whom became prominent interpreters of Japan for American audiences, is informed by his own cultural seduction while on a Fulbright fellowship in Yokohama in the 1970s. The book is self-conscious and personal in a way that deliberately defies conventional historical scholarship. Rosenstone announces himself interested not in how "We changed Them" but in "how They Taught Us" (p. xi); in answering that question, he takes himself as subject as well. His strategy is a kind of integrated literary montage—of diary entries, published accounts, fictive re-creation, and authorial speculation.

This is daring technique, which varies greatly in its success. As in more conventional history, the better the sources, the better the text. The great moments come with smart juxtapositions, as in Rosenstone's discovery of a moment of forgivable duplicity in Willie Griffis: the subject is the public baths, open to the streets, where both sexes companionably wash in easy acceptance of each others' bodies. Religious and puritanical by training, Griffis records in his journal his surrender to curiosity and his venture inside. But the published version of the same event has him the passive innocent—browsing in a bookshop while two young girls dart into a public bathhouse to avoid him, "afraid of a foreign face, yet unstartled by a dozen forms of masculine nudity" (p. 104). Rosenstone's meditation on what that deception means is a telling moment in his charting of Griffis's cultural odyssey.

Often, though, when Rosenstone is less well grounded, his fictionalizing voice becomes breathy and portentous. ("Excitement. Wonder. Disbelief. Those must be the feelings" [p. 10]). And his self-conscious metadiscourse—his writing about writing—often seems like mannered wheel spinning rather than profound illumination.

Excesses aside, much of Rosenstone's material is wonderful and provides just the *frisson* of cultural

recognition that he intends. We read about Hearn, the twisted, self-conscious loner, discovering the relief of Japanese impersonality. In Japan, there is less pressure for individual performance, which, after stifling American competition, grants a "psychic comfort" like emerging into "clear, free, living air" (p. 160). And there is Hearn emerging from the interior to find the jarring, intrusiveness of Western ways and a world "too full of men who work like slaves for no other earthly reason than that conventions require them to live beyond their means" (p. 243).

Ultimately, it is in such moments that Rosenstone's insight lies. He fights the scholarly expectation of reducible knowledge and prefers to leave contradictions at the level of paradox. At the end, of course, this Japanese utopia cannot hold up. Rosenstone's subjects must return to come to terms with their own culture and their relationship to it. For Rosenstone, the writing has been part of that coming to terms—exorcism accomplished. The result is intrepid, a risk worth reading.

JANE HUNTER
Colby College

MICHAEL J. MAZARR. *Semper Fidel: America and Cuba, 1776–1988*. Baltimore: Nautical and Aviation. 1988. Pp. 521. \$24.95.

Michael J. Mazarr's book is a history of U.S.-Cuban relations from 1776 to 1988 based entirely on secondary works. The specialist will not find any new interpretations or information in it. As a work for the general reader, however, this book is a mixed bag of useful material, errors of fact, and moralistic interpretation. Some chapters are informative and well balanced, while others are quite slanted and distorted.

For one thing, the book is much too long since Mazarr includes a variety of diversions from the basic story. Some material on Cuban history is useful, but the author hands the reader rather large chunks of it, mixed with dashes of sociology. For example, three chapters on the internal origins and developments of the revolution of Fidel Castro is excessive, and the lengthy discussion of John F. Kennedy's assassination adds very little to the broader analysis.

Another problem is the number of errors and significant omissions. Spain did not "negotiate the transfer of Texas" to the United States in 1833 (p. 36); Pierre Soule became ambassador to Spain in 1853, not during the James K. Polk administration (p. 38); Millard Fillmore did not serve as president in 1853 (p. 52; in the same paragraph the author correctly lists Franklin Pierce as chief executive); the Spanish *reconcentración* policy in 1896 was cruel but hardly guilty of "nearly annihilating the whole Cuban population" (p. 93); and, the Point Four program of the late 1940s was not formulated to assist U.S. corporations.

In his analysis of William Walker, Mazarr relies almost entirely on the works of William O. Scroggs

(published in 1914 and 1916) and completely ignores the later, and more informative, work of Albert Z. Carr. In fact, the author has a tendency to rely on one or two works for many chapters, and this dependence helps explain the uneven quality of his book. The latter chapters are heavily influenced by Wayne Smith's *The Closest of Enemies* (1987).

The author does not inform the reader that President Dwight D. Eisenhower moved to reverse the Cuban sugar quota only after the nationalization of the oil companies. In his discussion of the era of Richard Nixon, Mazarr says almost nothing about the efforts of Henry Kissinger to work out an accommodation with Cuba. Similarly, he barely mentions Nicaragua in his analysis of the Cuban policy of the Jimmy Carter and Ronald Reagan administrations. And, in his analysis of Castro's policies, Mazarr says nothing about the criticisms of such former political prisoners as Armando Valladares or Reinaldo Arenas.

The author continually stresses his attempt to write this history in a balanced, nonextremist way. And, to his credit, several chapters contain detailed comparisons of various interpretations. Yet Mazarr does have a fairly present-oriented interpretation that structures all Cuban history in the light of post-1959 developments. To Mazarr, Castro is the culmination of Cuban history, the end product of a peculiar, material dialectic, as it were, that has moved inexorably since 1492. In this process the United States becomes the *deus ex machina* well before 1898. As a result, Mazarr argues that the United States shaped almost every aspect of pre-1959 life, except for the radical, intellectual tradition that produced Castro. There is certainly some truth in this analysis, but too much is left out of this neat formula. Perhaps more than anything else Castro is a product of Latin-Catholic culture, implanted on a small, tropical island and mixed with twentieth-century totalitarian ideology. But, without massive economic and military support from the Soviet Union, this "inevitable" product of Cuban history and U.S.-Cuban relations might well be just another closed chapter in the history of the Caribbean.

ROBERT FREEMAN SMITH
University of Toledo

PHILIP BRENNER. *From Confrontation to Negotiation: U.S. Relations with Cuba*. (PACCA.) Boulder, Colo.: Westview. 1988. Pp. x, 118. Cloth \$24.95, paper \$7.95.

Philip Brenner's book is a mixture of scholarship and open advocacy. As scholarship it adds little to the field of U.S.-Cuban relations. It even contains a few serious errors, for example, citing Sumner Welles as the ambassador to Cuba at a time when he was actually an assistant secretary of state (p. 97) and transplanting a meeting of the National Security Council on Cuba to exactly one year before it occurred (p. 12), an error that affects the book's analysis of U.S. policy in 1959.

The work, however, is most appropriately judged as

a policy proposal. From this perspective it makes a more valuable contribution. The volume is part of a series of studies on U.S. foreign policy by Policy Alternatives for the Caribbean and Central America (PACCA), whose purpose seems to be, as William Appleman Williams once put it, to provide an "open door" to revolutions. Brenner's basic premise is that the almost unbroken thirty-year U.S. policy of isolating and overthrowing Fidel Castro's regime has not only failed but also been harmful to both Cuba and the United States. The constant threat of invasion has helped militarize Cuban society, has drawn off scarce resources for development, and has justified internal repression. Economic embargo has created an unwanted dependence on Soviet bloc markets. Moreover, the U.S. attempt to destroy the Castro regime drew Cuba closer to Moscow, alienated Washington from its allies, and introduced tensions into the inter-American system. In the absence of strong support either at home or abroad for a direct assault on the island, Washington turned to the shadowy world of covert warfare, engaging in sabotage, assassination, terrorism, and deceit. When these acts became known, the U.S. image as defender of international law, human rights, and the sovereignty of nations was seriously damaged.

Brenner presses home this progressive version of realism, charging that U.S. policy has actually harmed the national interest. Arguing persuasively, he suggests that the carrot rather than the stick would have produced much more favorable results: less Soviet influence in Havana, better U.S. relations with its allies (and especially the nations of Latin America), and an international environment more stable and lawful. The Cubans are shown as open, since the 1970s, to accommodation with the United States.

The author then assesses the possible fruits of honest diplomacy between the two states. Although he feels that serious ideological and political differences are likely to remain for many years, Brenner lays out a plausible scenario for achieving a significant lessening of tensions. He is weak, however, in his discussion of the Cuban side of the equation, especially the decision-making processes and goals of the Cuban elite.

The argument that negotiation rather than confrontation is in the U.S. interest is impressive but does not fully answer the charges from the Right that Castro is still a committed revolutionary expansionist or those from the Left that U.S. interests include the maintenance of unequal and unjust social systems in Latin America and the use of the bogeyman of communist subversion to distract North Americans from domestic problems. But the book is still the best summary we have of the advantages of peaceful coexistence with Cuba. It also contains at least brief discussion of the impediments to a change in policy: the history of U.S. hegemony, North American unwillingness to accept its loss of control over Cuba, and domestic constituencies who cynically use or who deeply believe in our responsibility to liberate the "enslaved" Cuban people.

I have one final caveat: policy realism (from what-

ever point on the political spectrum) is therapeutic but is not, in itself, a magic wand that will erase the heritage of U.S. hegemony or the Cuban apostasy. The influence of the United States was carried deep into Cuban society both by rarefied myths and by the solid structures of power. The rejection of these forces hurled Cuba far beyond the North American orbit. Simply punctuating these myths or attributing a healthy plasticity to the structures of power is not necessarily realistic. History relinquishes its hold only slowly, even to those who claim knowledge of the true national interest.

JULES R. BENJAMIN
Ithaca College

PAUL M. MINUS. *Walter Rauschenbusch: American Reformer*. New York: Macmillan. 1988. Pp. xii, 243. \$19.95.

Walter Rauschenbusch found himself between two worlds—America and Germany, ivory tower and Hell's Kitchen, thought and activism, word-filled work and soundless deafness. Perhaps it is from just such tensions and combinations that his creativity and power arose. During his fifty-seven years, Rauschenbusch studied theology, church history, and social reform in both Europe and America, pastored a German Baptist church in New York City, taught a wide variety of subjects at Rochester Theological Seminary, wrote and lectured extensively on what came to be known as the Social Gospel, and sought peace in a world racked by war. In spite of his eminence, especially as the preeminent figure in the creation of the Social Gospel, Rauschenbusch had been the subject of only one full-scale biography, written by Dores R. Sharpe in 1942. Paul M. Minus now offers an additional perspective on this significant religious leader.

Minus notes in his introduction that his work is intended as a study of the life of Rauschenbusch, not as an exposition of Rauschenbusch's thought or an analysis of the Social Gospel as a whole. True to that promise, Minus summarizes childhood, schooling, pastoral and seminary work, marriage, and family life. Writings and the disputes that they engendered are summarized briefly. The final product is a work of admiration and affection, apparently a gift of thanks for inspiration in the author's own life and an offering to others who might follow in the Rauschenbusch tradition.

Historians will find themselves calling for more critical analysis, however, especially in areas where Rauschenbusch might be cast in a negative light. For example, Minus admits that Rauschenbusch followed contemporary theories on the unity of a Teutonic race destined for world leadership (p. 107). That way of thinking may be all too easily passed over simply as evidence that Rauschenbusch was a man of his age. Such racial theories must be crucial, however, to understand fully the thought of a man who concentrated on the problems of immigrant-filled slums and of a

capitalist system built so much on the labor of non-Teutonic workers. Similarly, Minus recognizes the relationship between Rauschenbusch and the Rockefellers—a friendship that included regular and substantial financial contributions from the wealthier family to the pastor and professor's household (pp. 97, 133). Elsewhere, he notes that Rauschenbusch carefully distinguished between the evils of the capitalist system and the potential goodness of individual capitalists (pp. 168–69). Yet Minus refuses to make any connection between the personal experience and the theory but avers only that the money came from the Rockefellers with “no strings attached” and that there is no evidence in the correspondence of the two families that points to an effort to create a dependency or to soften Rauschenbusch's criticism (p. 134). One does not need to accuse the religious leader of succumbing to base bribery in order to connect Rauschenbusch's experience of the personal warmth and helpfulness of one of the age's premier capitalists with his conclusion that capitalists could be revered while capitalism was excoriated.

A new biography of Rauschenbusch may be taken as an object lesson in the opportunities available if historians of religion and historians of the “secular” would be more willing to cross over onto one another's turf. Rauschenbusch, the admirer of Henry George, the proponent of gas-and-water-socialism, and the friend to the progressive business leaders of Rochester, is a fit subject through whom a fuller understanding of late nineteenth-century reform and of the rise of Progressivism might be approached. Historians of the Progressive era might do well to bring him out of the ghetto of their separate chapters on the Social Gospel; religious historians might brave the effort to put him in the context of broader changes around the turn of the century. Minus chooses to do neither of those things. His work fits on the bookshelf next to that of Sharpe, whose biography Harry Emerson Fosdick called “a ministry of recollection” (Fosdick, “Introduction,” in Sharpe, *Walter Rauschenbusch*, p. xi).

RUTH ALDEN DOAN
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KAY SLOAN. *The Loud Silents: Origins of the Social Problem Film*. Urbana: University of Illinois Press. 1988. Pp. ix, 166. \$24.95.

Fortunately for American cultural criticism, since analysis of motion pictures adds a significant dimension, the beginnings of the American film industry coincided almost exactly with the Progressive era. Not surprisingly, film makers in those freewheeling days before the coalescence of the studio system ransacked the drawers and cupboards of American life to gain stories for their two-reelers.

The result, the meeting of the social concerns of Progressivism with the nascent and profit-oriented film industry, is the subject of Kay Sloan's short but mea-

sured and informative study. Using evidence drawn from industry periodicals and the relatively few surviving silent films, she argues that commercial films presenting the contemporary issues of progressive reform were "cultural signifiers" that played complex roles in political and social problems (p. 20).

The book is thematically sectioned into examinations of films depicting political corruption, class struggle, labor unionism, sexual politics, and woman's suffrage. The author is at her best in the early chapters, where she makes a convincing case that movies actually were upholding middle-class versions of order while seemingly depicting its dissolution. Ultimately, the conflicts presented in the era's films tended to be adjudicative and controllable. Far from a cinema of revolution, this entertainment-oriented, pre-Hollywood product was at best a cinema of moderate reform, and so it has continued to be ever since.

Well-known reform figures—Jacob Riis, Upton Sinclair, Jane Addams, and Emmalene Pankhurst, among them—dabbled with film either directly or indirectly. D. W. Griffith made his early reputation with sappy renditions of family and political problems, but Sloan correctly presents him as less a reformer than a classic Victorian romantic, a far cry from reformers such as Margaret Sanger who deliberately sought to use film to further their cause.

Melodramatic convention was made for political and social strife, and Sloan's films (she examined over two hundred in all) commonly intercut early newsreel shots of industrial struggle with their four-handkerchief plots. Even in the most militant settings, though, film still managed to minimize economic differences and class struggle, emphasizing instead the eternal verities of love and marriage. In this sense, older romantic forms were the first fruits of modern technology, a strong indication of cultural continuity.

The film industry itself was unabashedly bourgeois despite its early appeal to urban working-class audiences. Thus, the entrepreneurs, little troubled by any sense of hypocrisy, would preach, rage, uplift, invoke, or bemoan as their sense of public sentiment and the state of their cash registers moved them. Sloan shifts somewhat from the relationship between this determinedly middle-class, mass-market industry and the acute sociopolitical problems of the day when she considers films concerning the "woman question." In this regard the reader finds that films displayed the "utopian fancies of a repressed society" (p. 79), that the United States of the Progressive era was a "sexually repressed nation" (p. 84), and that the film industry, in dealing with sexual and political conflicts, "exposed the insecurities of a nation of moviegoers" (p. 123).

Most of the problems with which these films dealt, such as birth control, alcoholism, and suffrage for women, were real and of crucial historical importance. One—white slavery—was largely a publicity hype to which the author devotes far too much space. Suffrage films, in particular, never proved to have mass appeal,

however; they went too strongly against the grain of romanticized womanhood.

Despite a tendency toward overstatement and an exasperatingly vague analysis in her concluding chapters, Sloan has produced a strong, well-organized study of the socially oriented film produced in the early years of the motion picture industry. Her "cultural signifiers" make excellent historical evidence.

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GEORGE H. NASH. *The Life of Herbert Hoover. Volume 2, The Humanitarian, 1914–1917*. New York: W. W. Norton. 1988. Pp. xii, 497. \$25.00.

This volume is the second of George H. Nash's planned multivolume biography of Herbert C. Hoover. Like the first one, it is an immense contribution to scholarship on Hoover and is more a document of sources on a phase of Hoover's life than an interpretative narrative of the same period. The effort of Hoover recounted here is his work with the Commission for the Relief of Belgium (CRB), 1914–17. In minute detail, the author, having scoured all imaginable sources, describes his subject's brilliant though brusque leadership in his organization and administration of the CRB (with some attention given to Hoover's 1914 repatriation of Americans stranded in Europe and his appointment in 1917 as Woodrow Wilson's food administrator.) Although this volume is very important to a definitive understanding of the massive World War I famine relief efforts and of Hoover's organizing genius, it is, by its subject matter, a less engrossing book than the author's first volume, which described Hoover's youth and mining career. And the topic is surely less interesting than subsequent volumes will be, which will put Hoover into the American bureaucratic and electoral political context. Thus, the book introduces readers to a myriad of Hoover's bureaucratic convolutions and his intrigues in playing off the leadership of England against that of Germany as the CRB plied the food and clothing supply routes to Belgium and oversaw the distribution of relief. Aside from Hoover's executive abilities and strategies, the relief program succeeded in large part because the Germans did not have to feed the Belgians and because the British viewed their role as enhancing Belgian passive resistance against the Germans. The Allies, for obvious reasons, also cooperated in order to "court" (p. 193) the United States. But Hoover justifiably received the credit for feeding nine million people. President Wilson noted, "He is a real man . . . one of the very ablest we have sent over there—a great international figure. Such men stir me deeply and make me in love with duty" (p. 150).

The book is replete with Hoover's various devices and schemes aimed at any political leaders who got in his way—for example, British prime minister Herbert H. Asquith; the Spanish minister to Belgium, the marquis of Villalobar; Belgian banker Emule Francqui;

the German governor-general in Belgium, Moritz Ferdinand von Bissing; and the U.S. minister to Belgium, Brand Whitlock. Although Hoover masterminded the relief effort, he did not do it alone. He had under him a band of intensely loyal aides, many of whom continued to serve their "chief" in his long life, and, most important, he had the enthralled support of Walter Hines Page, the American ambassador to Great Britain, who consequently was described by Hoover as "the soul of intellectual integrity" (p. 150).

Although one readily perceives Nash's admiration for his subject, one must appreciate not only his description of Hoover's energy, will, mental concentration, reserve, assessment of people, and dominating personality but also the author's candor about Hoover's tendency to such behavior as tactlessness, "retroactive melodrama" (p. 26), misstatements of facts, exaggeration, half-truths, and trickery. (The author, in an aside regarding Hoover's violation of a mining contract agreement with Bewick, Moreing, and Company, notes, "In short, Hoover had not simply defeated his former partners in honest competition. He had intrigued, both illegally and unethically, to destroy them." Furthermore, "Hoover's own papers contain nothing about . . . [the] lawsuit" [p. 268].) Of course, deceit, at times, is a political imperative, but the author's unabashed allusions to it confirm my assumption (in my *Presidency of Herbert C. Hoover* [1985]) that Hoover's highly successful bureaucratic politics (further enhanced subsequently as food administrator and secretary of commerce) in large part doomed any electoral political role he might perform, such as president of the United States. It was one thing for Hoover to work under bureaucratic cover and quite another to be employed by what he himself came to describe as a harsh employer, namely, American democracy.

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LOUIS P. CASSIMATIS. *American Influence in Greece, 1917-1929*. Kent, Ohio: Kent State University Press. 1988. Pp. xiii, 300. \$25.00.

In this superb study, based on extensive archival research in Athens, London, and Washington, D.C., and on numerous published documents and studies, Louis P. Cassimatis clearly demonstrates the asymmetry between the publicly stated U.S. isolationist policy and the domestic economic forces that compelled Washington to become financially and diplomatically entangled in European and Near Eastern affairs from the Woodrow Wilson to the Herbert Hoover administrations. World War I created propitious commercial and financial opportunities for U.S. interests in some traditional European spheres of influence, and Greece arguably represents the quintessential paradigm of this general trend.

Between 1914 and 1921, Greek-American trade in-

creased by 1000 percent, and Greek statesmen from both the Liberal (Venizelist) and monarchist camps began looking to Washington not only for loans, trade, and capital investment but for diplomatic support as well. Cassimatis contends that, despite traditional interpretations of U.S. interwar foreign policy, President Wilson's multilateralism was not totally discarded in the euphoria of postwar bipartisan isolationism. On the contrary, the United States took advantage of its global ascendancy both to protect existing commercial interests and to augment them in the Eastern Mediterranean, where the French and the British traditionally had exerted hegemony. Arguing that the Tripartite Loan of 1918 "served as the catalyst in the evolution of Greek-American political relations" (p. 6), Cassimatis places relations between the two countries in the broader Near Eastern context to demonstrate Washington's resolve to challenge entrenched Anglo-French commercial influence in the Mediterranean.

Although the book is well balanced, the author tends to overdramatize certain issues to strengthen his thesis. One of the more salient points is Greece's geopolitical and commercial importance to Washington's Near Eastern policy. Although there can be no question about the increased U.S. financial and commercial involvement and conspicuous naval presence in the Eastern Mediterranean, Cassimatis does not make a convincing argument for "the connection between the Monroe Doctrine and the [Near Eastern] Open Door policy, as it concerns Greece" (p. 66). He supports this position by arguing that the State Department demanded commercial and financial concessions both from the Greeks to recognize King Constantine and from the Mexicans to recognize President Alvaro Obregón and the new "revolutionary" regime in that country. Although this point certainly illustrates the significance that Washington attached to its overseas economic interests, it would be less than accurate to place Greece and the Near East on a level with Mexico and Latin America, which were historically of paramount importance to the United States.

In tracing Greek-American relations from World War II to the demise of the military junta in 1974, Cassimatis reaches the politically realistic and sound conclusion in the epilogue that Greek "dependence [on the United States] must be replaced by interdependence, and a true diplomatic partnership must be established" (p. 212). Recent studies on Greek-American relations concur with the position that Greece is indeed a paradigm of a small country seeking to elevate itself in a rapidly changing polycentric world by renouncing the anachronistic bipolar politics of the cold war and embracing the politics of constructive coexistence at the regional and international levels.

Cassimatis has made a valuable contribution not only to modern Greek and Near Eastern history but to American foreign policy studies as well. This engaging and thoughtful book will be of interest to readers of diplomatic history and to future researchers wishing to

reexamine traditional interpretations of U.S. interwar diplomacy in the Near East.

JON V. KOFAS
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MAURER MAURER. *Aviation in the U.S. Army, 1919–1939*. (General Histories.) Washington, D.C.: Office of Air Force History, United States Air Force. 1987. Pp. xxxiii, 626. \$29.00.

Maurer Maurer, now retired after thirty years as a historian with the U.S. Air Force, has filled the gap between his earlier books on the two world wars with this encyclopedic account of army aviation in the interwar years. He traces the halting evolution from the concept of an air service, which implied an auxiliary function, to an air force, suggesting more independent operations.

The high point of the transition came in 1934 when, on ten days' notice, the air corps took over carrying airmail for more than three months. Air corps chief Benjamin Foulois agreed to the operation and raised considerable controversy as undertrained and poorly equipped air corps pilots died heroically fighting winter weather and malfunctioning or nonexistent basic equipment. The accidents were timely in a bureaucratic sense, for Congress was in the process of making appropriations, and the obvious need brought a \$5 million windfall to the air corps. Foulois regarded the airmail episode as a milestone, rejecting allegations that it was a fiasco or a disaster.

Maurer's book shares the strengths and weaknesses of institutional histories. On the positive side, the Government Printing Office has outdone itself with heavy clay paper, numerous illustrations (including an appendix with pictures of the planes discussed in the text), foldout maps with colored highlights, and ten appendixes for quick reference. On the other hand, Maurer had completed his work by 1981. The reference features appear to have been added during the six-year gap before publication; unfortunately, they are not well-keyed to the text and lose much of their value as a result. Among other questions, why is the interesting map of airline routes in 1934 included? Why is the assistant secretary of war for the air force not listed in the appendix when every other pertinent official is? Why is there not a note in the text guiding the reader to the aircraft photographs in the appendix when first appropriate? The apparent assembly by committee weakens the book.

Maurer notes in his introduction that he wants to be descriptive rather than analytical and to focus on specifics rather than generalizations. It may be unfair to criticize the book for what the author did not intend, but he could have strengthened it considerably with more analysis. He would also have done well to convey more passion for his cast of characters.

He is at his best in relating intrepid attempts to set

distance and altitude records. In 1927, Captain Hawthorne C. Gray twice strove bravely to set American and world altitude records in balloons. His first attempt took him to 42,470 feet, a world record, but he had to bail out while descending when his balloon did not form a descent-slowng parachute shape. The picky Federation Aeronautique Internationale (FAI) denied him an official record because he bailed out. On his next try, Gray again reached 42,470 feet, but his bid for an official record was again denied by the FAI because he "was not in personal possession of his instruments" (p. 271)—because he died from lack of oxygen on the way down. Even in the face of such heroics, Maurer remains dispassionate. Perhaps as a result, the reader never gets to know either people or aircraft as more than casual acquaintances.

Strangely, in a copiously documented book relying mostly on primary materials, Maurer has overlooked some secondary works of importance. Nick Komons's *Bonfires to Beacons* (1978) treats the growth of the government role in civil aviation during the same period and offers some useful insights on the common civil and military technological advances. My dissertation, "Herbert Hoover and the Armed Forces" (1971), treats interservice rivalries and cooperations. Maurer seems unaware of either work.

Despite such caveats, Maurer has written what will likely be the final word on his subject for many years. The extent of his research and length of his work suggest that few historians will soon seek to supplant it.

JOHN R. M. WILSON
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W. DAVID LEWIS and WILLIAM F. TRIMBLE. *The Airway to Everywhere: A History of All American Aviation, 1937–1953*. Pittsburgh: University of Pittsburgh Press. 1988. Pp. ix, 230. \$27.95.

This is a well-researched and gracefully written book on a narrowly focused topic. W. David Lewis and William F. Trimble take as their subject the founding and early development of All American Aviation, the corporate ancestor of today's giant carrier, USAir. Lewis and Trimble begin their story by recounting the career of Lytle S. Adams, an erstwhile dentist turned inventor. In the late 1920s and 1930s, Adams obtained patents for a device that permitted a plane to pick up and drop mail or any other small cargo without landing. Envisioning the new technology as a means of bringing air mail to the entire population, Adams by 1937 had persuaded investors to back the idea of aerial pickup.

Yet efforts to exploit the concept of aerial pickup encountered many problems. Adams's invention required further development before it was reliable, and there was little demand for air mail service in the numerous small towns to which All American intended to fly. Most important, All American, like traditional air

carriers during the period, could not balance their budgets without government subsidies.

Lewis and Trimble are at their best in meticulously delineating the changing environment of government subsidy and regulation in which All American existed. They argue that Adams's aggressive lobbying for government support of his pickup technology catalyzed the passage of the Experimental Air Mail Act of 1938. That legislation authorized pickup service and ultimately led to the award of two such routes to All American. The company had barely begun to realize Adams's vision of bringing air mail to the people, however, when war broke out in Europe, prompting the suspension of pickup service. But the war stimulated air mail along with all aeronautical activity, and the company prospered, hauling mail and packages for the U.S. Air Transport Command and, through the work of its Manufacturing and Development Division, perfecting means whereby gliders and even downed fliers could be picked up using Adams's technique.

After the war, subsidies and war contracts ended. If All American were to survive, it would require profitable routes, which meant favorable treatment by the Civil Aeronautics Board (CAB). In 1948 the CAB, bent on strengthening passenger service on trunk carriers while developing regional feeders, finally denied on safety grounds All American's longstanding petition to fly passengers on its pickup flights. The CAB, however, did award the company six conventional passenger routes in the Middle Atlantic region where it had pioneered airmail pickup service. By July 1949 American Airways, with its name changed to signify the fact, became an ordinary passenger-carrying airline. Lewis and Trimble follow the new firm up to 1952, when the Engineering and Development Division was split off as All American Engineering and the airline again changed its name to Allegheny Airlines, Incorporated, the direct parent of USAir.

As a narrative history of a single firm, this work displays faults common to the genre. For one thing, it does not set its topic in comparative perspective. Readers might well ask, for example, how All American's experience in operating its particular aircraft, training its pilots, or managing risks compared with that of other carriers. Second, the book at points reads like an in-house history, lionizing All American's employees and aircraft and taking excessive pride in the company's achievements. Those criticisms notwithstanding, the authors have made a solid, if modest, contribution to our understanding of the interaction of technology, business, and government in the aeronautical world of the 1930s and 1940s.

JOSEPH J. CORN
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JAMES J. FLINK. *The Automobile Age*. Cambridge: MIT Press. 1988. Pp. xii, 456. \$25.00.

This book, the latest, longest, and most significant of James J. Flink's three studies of the automobile's changing place in American society, explicitly extends and revises ideas developed in his earlier works. In 1970 Flink published *America Adopts the Automobile, 1895-1910*, which, though hardly the initial scholarly analysis of the automobile's origins, in many respects was (and still is) the best of those pioneering works. Like his later books, it successfully integrated the "nuts and bolts" of the automobile's early days—the principal focus and approach of most prior automotive histories—with the then new social history.

Given not only the short span it covered but also its adulatory attitude toward automobiles, *America Adopts the Automobile* barely hinted at the emergence of what his next work termed, by its title, *The Car Culture* (1975). By this Flink meant not simply a culture surrounding the car, as manifested in Americans' long-time obsession with personalizing and flaunting their vehicles, but, more important, a cultural force virtually shaping the rest of American life, from the layout of cities and suburbs to the patterns of work and leisure within them.

The Car Culture admittedly incorporated portions of the first book to cover the automobile's origins, while also providing much new material to bring the story up to the early 1970s. Moreover, it appealed to those "under thirty" (p. ix), whose criticisms of the capitalist, corporate ethos associated with the American auto industry Flink increasingly shared. Consequently, he rejoiced in as well as argued the car culture's end, a change allegedly analogous to the closing of the frontier announced by Frederick Jackson Turner. He pleaded for an "alternative future . . . free from the privatism, materialism, escapism, and exploitation" (p. 233) of what he presently calls *The Automobile Age*.

The new book is remarkably free from the polemics that undermined *The Car Culture*, particularly as the younger generation praised there has grown older, more materialistic, and less socially critical. If anything, *The Automobile Age* is favorably disposed toward the contemporary American automobile industry insofar as it has finally produced smaller, safer, less polluting, more fuel-efficient vehicles that can compete successfully with foreign imports. As in his earlier works, here Flink writes clearly, provides apt examples and quotations, and is in command of the scholarly literature. The repeated drifts between chronological and thematic developments and the lengthy biographical interludes that slowed the reading of *The Car Culture* are both happily missing from this more cohesive work.

The book focuses on the evolution and refinement of automotive technologies, mass production techniques, business organizations, labor-management relations, marketing strategies, consumer consciousness, government regulation, and, not least, the car culture itself. Here the car culture is not the uniformly negative force it was in 1975, yet it can still supposedly explain much of modern American history and indeed that of other advanced capitalist nations.

Given the car's "new lease on life" (p. 404) since 1975, it is not surprising that Flink qualifies his obituary for the "Automobile Age." As a force for progress, it has been dead for decades. As an economic and social force in America and elsewhere, however, it may yet thrive into the next century—aided by various technological advances ironically developed by other industries. In any case, *The Automobile Age* stands as a revisionist history in the best sense: a tribute to its author's intellectual maturity and the foremost study of its kind.

HOWARD P. SEGAL
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DAVID A. HOUNSHELL and JOHN KENLY SMITH, JR. *Science and Corporate Strategy: Du Pont R&D, 1902–1980*. (Studies in Economic History and Policy: The United States in the Twentieth Century.) New York: Cambridge University Press. 1988. Pp. xx, 756. \$39.50.

In this massive study, David A. Hounshell and John Kenly Smith, Jr., use their extensive access to Du Pont's corporate files to craft the most thorough analysis to date of a modern corporation's research and development efforts. They draw on the pathbreaking studies of Reese V. Jenkins, George Wise, and Leonard S. Reich, who focused on the research and development pioneers of Kodak, General Electric, and American Telephone and Telegraph. Yet Hounshell and Smith cast a wider net in time and particularly in the scope of their study. Their analysis of Du Pont from the inception of its first research laboratory in 1902 to 1980 focuses on external as well as internal pressures on research and development. Thus, they discuss the complex origins of such products as nylon, orlon, and teflon, while interweaving the evolving relationships between scientists and managers and their interaction with the market, the scientific community, the government, and international developments. Hounshell and Smith also investigate the commercialization of several products to explore the coordination of research and development with manufacturing and marketing. This synthetic approach thus provides a clear picture of the changing complexities of modern industrial research and development.

Unlike General Electric, American Telephone and Telegraph, and Kodak, which organized research laboratories to protect their technological leads from competition, Du Pont's managers founded the Eastern Laboratory in 1902 to consolidate the explosives industry as well as to develop and improve products scientifically. The Experimental Station in 1903 received a broader mandate, especially after a perceived threat by the U.S. military prompted Du Pont to conduct fundamental research to convince the government that private research and development was "progressive" and should not be challenged. The success of this effort, but particularly the outgrowth of nylon and neoprene from fundamental research on polymers in the 1920s

and 1930s, established one of the continuing debates in Du Pont's offices: whether to concentrate more on applied or on fundamental research. Another debate had roots in the two research laboratories but intensified after the decentralization of the corporation in 1921: whether a central research department or many units tied to respective divisions was best. As Hounshell and Smith show, Du Pont's leaders chose both options in these debates. The firm also wavered between buying technologies and improving them or generating them internally. After the success with nylon and post-World War II antitrust prosecution, the nylon model appeared most promising. Faced with increasing competition and a maturing industry, however, Du Pont began by the late 1960s to question its strategy of high risk and high profit innovation. The late 1970s brought both a new major effort in the life sciences and an expanded base with the acquisition of Conoco.

Although this book was originally the idea of Du Pont's chairman and chief executive officer, and the firm financed much of the project, Du Pont gave the authors complete access to the company's records and assurances of academic freedom. The generally favorable tone of the work will still trouble some readers, however. The authors' masterful use of the far greater sources unavailable to others and their interpretation of complex scientific, technical, and business developments result in what will become the standard work on Du Pont's research and development efforts. A couple of matters, however, concern me. Hounshell and Smith tend to portray the firm as reacting to, and not interacting with, the government on matters relating to research and development policy. Du Pont's dodge of the military's challenge in 1903 thus requires investigation in military documents to explore the relationships between Du Pont's and the military's technical staffs. The authors should also explore the interaction between officials of Du Pont and the government in such related activities as patent policy and the increasing environmental regulations of the 1950s–1970s. Another disappointment is the authors' unwillingness to form a more comprehensive theory of the relationship between corporations and scientists, even though they are fully aware that their account and evidence support in part several current interpretations (for example, those of Leonard Reich, George Wise, and David F. Noble). These problems pale, however, in the face of the authors' impressive accomplishments.

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DANIEL NELSON. *American Rubber Workers and Organized Labor, 1900–1941*. Princeton: Princeton University Press. 1988. Pp. x, 339. \$32.50.

Daniel Nelson's expertise in the history of industrial management in the United States equipped him well to write a detailed and persuasive study of the efforts of

rubber workers to create durable and effective unions during the first four decades of this century. Based on thorough research in government archives, company and union records, and newspapers (especially those of Akron, Ohio), the book is a densely packed narrative with detailed accounts of many strikes and splendid biographical sketches used to open chapters. Nelson engages only rarely and almost cryptically in debates with other historians.

His analysis emphasizes the economics of the rubber industry and the quality of leadership of the companies, the unions, and government. The industry consisted of a tire sector, concentrated in Akron and dominated by four vertically integrated corporations, and a highly diversified rubber products sector, much of which was scattered in smaller firms around the land and which had technology, labor forces, and patterns of expansion and contraction very different from those of the tire companies. The Akron corporations' dependence on sturdy farm youth for tire builders and pit men made their city "the capital of West Virginia" after World War I, bred endemic sit-down strikes over piece rates, and encouraged managers to pioneer in welfare programs, elite corps of loyal employees, and company unions. No other study of twentieth-century workers offers such rich accounts of the operations of employee representation plans or comparable attention to their resilience even after contracts had been signed with the Congress of Industrial Organization (CIO). The new business policies of dispersing tire production out of Akron, which began in the 1930s and whose devastating climax a generation later lurks like an unspoken premise behind Nelson's assessment of the CIO, are also subjected to careful scrutiny.

Although the author makes several references to the "labor culture" of Akron, he offers little analysis of its content, aside from some discussion of the Socialist party after 1913 and the impact of middle-class antagonism toward the sit-downs of the 1930s in defeating labor's ventures into municipal politics. Discussions of the Ku Klux Klan, the Stahl-Mates, and Goodyear's Flying Squadron are informative but tantalizingly brief. The decisive importance of the New Deal in Nelson's view was the opportunity it provided union leaders in the large factories to develop their skills and tactics before they faced tests that had crushed earlier union efforts.

DAVID MONTGOMERY
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BRUCE NELSON. *Workers on the Waterfront: Seamen, Longshoremen, and Unionism in the 1930s*. (The Working Class in American History.) Urbana: University of Illinois Press. 1988. Pp. xiii, 352. \$29.95.

During the early years of this century, two major developments—the rise of mass production manufacturing and the expansion and mechanization of service industries—transformed the labor force, creating

marked distinctions between old- and new-style employment. Work in ocean transportation was decidedly old-fashioned. On ships and docks, small corps of highly skilled workers directed larger groups of laborers. Men worked by the job, rarely knew their employers, and in many cases had no settled home life. Wages and working conditions were extremely poor. Many sailors and longshoremen were adventurers, rebels, or transients. Tough and alienated, they were nevertheless indispensable to the movement of goods by water. If they worked together, they could wield considerable influence.

Bruce Nelson analyzes their behavior during the 1930s when a combination of depression, recovery, and government support for collective bargaining made it possible for sailors and dock workers to organize and use their latent power. Focusing on the Pacific Coast maritime trades, Nelson reviews the major events of the decade, in particular the Great Strike of 1934, the prolonged strike of late 1936 and early 1937, and the rise and fall of the Maritime Federation, an informal alliance of Pacific Coast maritime unions. He also discusses improvements in wages, working conditions, and employment security that were prompted by unions and that, at least temporarily, gave maritime workers a glimpse of modernity. Nelson is not really interested, however, in industrial relations or the economic effects of unions, nor, for that matter, in rank-and-file workers, who remain a blur. His most notable contribution is his description of the leadership styles and policies of the men who took charge of the new and revived maritime unions.

Two patterns, which had deep roots in the maritime subculture, characterized the union impulse of the 1930s. Angry workers turned to radicals, men who had been active in the Industrial Workers of the World and in many cases were members of the Communist party, or they fell under the sway of gangsters, who reflected the other side of the militant tradition of the docks. The distance between those divergent paths was actually quite narrow. As Nelson points out in his most telling portrait, Harry Lundeberg, the leader of the Sailors Union of the Pacific, started as the most flamboyantly radical of the maritime union leaders and evolved into a gangster. Harry Bridges, the best known of the Pacific Coast militants, took the other fork, becoming a dutiful Communist functionary by the end of the decade. Although Nelson strongly favors the Communist option, he is scrupulously honest in portraying its shortcomings and vulnerabilities. Even during the heady years of the 1930s, when everything seemed to be possible, the character of maritime work and the legacies of earlier years severely circumscribed the potential of the maritime organizations.

Although sailors and longshoremen were outside the mainstream of union activity in the 1930s, their actions cast light on two important interpretive issues. First is the role of Communists in the labor movement, a topic that has received much attention in recent years. Communists were active in all the maritime organizations,

exercised substantial influence in Bridges's longshoremen's union, and became an issue in the vicious internecine union rivalries that racked the Pacific ports during the later 1930s. But did they matter? Nelson's reluctant answer is that the Communists mattered very little during the early years of union building and seriously hurt the maritime organizations during the late 1930s, when they became targets for antipathy toward unions and the Congress of Industrial Organizations. Nelson persuasively argues that worker militancy was the key to maritime union activity; Communists became and remained leaders only if they personified that militancy, regardless of party line or personal conviction. If they had not appeared, others equally radical presumably would have stepped forward. Once the Communists gained power, they were financially honest and sensitive to their constituents' demands. Their political initiatives amounted in practice mostly to symbolic gestures, such as demonstrations against Nazi Germany. Considering the alternatives—men like Lundeborg or out-and-out criminals—it is not hard to understand the Communists' durability in the face of mounting public anxiety.

The behavior of maritime workers also highlights the relationship between worker militancy and the evolution of the labor movement. Nelson vividly describes the many manifestations of rank-and-file activism in the maritime industries but adds surprisingly little to the larger story of worker militancy in the 1930s. Indeed, the most notable weakness of his account is his penchant for generalities when more specific, detailed information seems essential. The results of the activities he describes are clear, however, and do not differ substantially from the results of similar uprisings in more modern industries. In that sense, at least, the experiences of the 1930s gave sailors and longshoremen something in common with workers in factories, offices, and stores throughout the land.

DANIEL NELSON
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BENJAMIN KLINE HUNNICUTT. *Work without End: Abandoning Shorter Hours for the Right to Work*. (Labor and Social Change.) Philadelphia: Temple University Press. 1988. Pp. x, 404. \$34.95.

Given the prominence of the shorter workday in nineteenth- and early twentieth-century political movements (and social theory), the absence of a modern history of the drive for shorter hours has been notable, indeed glaring. Benjamin Kline Hunnicutt has attempted to address this neglected topic: his book is an account of the final chapters (thus far) of the shorter hours story, an effort to explain why, in the 1930s, American society abandoned the goal of shorter hours in favor of the objective of job creation. Hunnicutt shapes his account in terms of a dichotomy, or tension, between work and leisure. The goal of the book, accordingly, is to explain why Americans made the

rather puzzling choice of work (linked to economic growth, job creation, and consumerism) rather than leisure (linked to nonpecuniary rewards, play, and creativity) at just the historical moment when technology was making widespread leisure more possible.

Much of the study is, in effect, an intellectual history of the movement for shorter hours, with a particular emphasis on leisure as the goal of a shortened workday. Hunnicutt briefly summarizes known features of nineteenth-century movements, and he explores in some detail the era between 1900 and 1920, when the hours of work in most occupations were sharply curtailed. But his focus is on the years between 1920 and 1940, and here he examines at length the views of (among others) business leaders, labor leaders, educators, psychologists, reformers, and librarians. According to Hunnicutt, interest in the shorter workday for the sake of leisure—and to reduce unemployment—peaked in the early New Deal, when legislation calling for a thirty-hour week came close to being enacted.

But that legislation was not enacted, and the New Deal turned decisively away from shorter hours and toward the goal of guaranteeing the "right to work." The second half of Hunnicutt's book is an effort to explain that turn of events, to chart, in great detail, the political and intellectual history of all federal legislative efforts bearing on the length of the working day and the quantity of leisure. Hunnicutt sees key New Deal figures (including Franklin D. Roosevelt) as antagonistic to shorter hours, and he reinterprets the legislative histories of the National Industrial Recovery Act, the Social Security Act, and the Fair Labor Standards Act accordingly. He also analyzes, at length, the ideas of key actors in the political drama, such as Rexford Tugwell, and of scientists and engineers, such as Arthur Dahlberg. The book closes with a chapter entitled "The Age of Work," which includes a meditation on two films of stories by H. G. Wells and which resonates with regret that golden opportunities for American society to espouse less work and more leisure were lost in the 1930s—perhaps irretrievably.

This is a useful book that mobilizes an unusual perspective (Hunnicutt is a professor of leisure studies) to examine a central phenomenon in the history of American political economy. Whether that perspective satisfactorily illuminates the subject is, however, open to question: I, at least, am not convinced that the dichotomy between work and leisure adequately or effectively accounts for the developments Hunnicutt describes. Hunnicutt, moreover, pays too little attention to the economic and social forces that shaped the choices available to policy makers, and, by overemphasizing the role of leisure, he ignores some of the very real economic and social problems that might have been spawned by a significant reduction in the workday during the 1930s. Although students of shorter hours will certainly want to consult it, this detailed, indeed

overly detailed, narrative will not be the last word on the subject.

ALEXANDER KEYSAR
Duke University

JOHN A. SALMOND. *Miss Lucy of the CIO: The Life and Times of Lucy Randolph Mason, 1882–1959*. Athens: University of Georgia Press. 1988. Pp. xiii, 227. \$24.00.

In this study, John A. Salmond explores the story of Lucy Randolph Mason's half-century of struggle for social justice in the American South. She devoted herself to a broad range of concerns, including the labor movement, the struggle for civil rights, feminism, and the peace movement. Mason also felt deeply about such issues as child labor, suffrage, and prison reform, and she believed in the institutions that fought for them—the Highlander Folk School, the Southern Conference for Human Welfare, the National Consumers League, the Young Women's Christian Association (YWCA), and the Congress of Industrial Organizations (CIO).

Salmond creates a detailed portrait of Mason as a woman of distinguished lineage. Her ancestors included George Mason, a signatory to the Declaration of Independence and author of the Virginia Bill of Rights; John Marshall, the first chief justice of the Supreme Court; and Robert E. Lee, her father's second cousin. She could have spent her life in a narrower, more sheltered world, one more typical to women of her class and time. Salmond makes clear throughout that Mason was in one sense an unlikely ally of the causes for which she worked so tirelessly. At the same time, however, Salmond adds to Mason's story by taking care to place her in the context of women social reformers of the late nineteenth century. As the daughter of an Episcopalian minister, Mason grew up with a commitment to the Social Gospel. She carried those impulses into the new terrain represented by groups such as the National Consumers League, the YWCA, and the CIO.

Another of Salmond's contributions is a clear view of the political realities of the South. One of the problems that students of the post–World War II South face is the political complexity that grows out of a certain southern simplicity: racism. The multiple variations of white supremacy alive in the region have historically struck many white southerners as the very essence of politics, the “one thing” that had to be addressed. This attitude had the practical effect of squeezing all kinds of white southerners under one broad umbrella, bringing into a sometimes uneasy alliance so-called moderates, liberals, socialists, and communists, to name a representative sample. Thus, a wide variety of southern political activists, including Mason, laid themselves open to attack as “communists” or, at the very least, “fellow travelers.” The labels sometimes fit, sometimes did not, leaving a challenging maze for historians to sort out. One of the great strengths of Salmond's work

is his poise in coping with this task. His book is a substantial addition to the growing literature on that admirable band of southerners, black and white, who took a different kind of stand in Dixie.

BARBARA S. GRIFFITH
National Museum of American History

JOANNE BENTLEY. *Hallie Flanagan: A Life in the American Theatre*. New York: Alfred A. Knopf. 1988. Pp. xviii, 436. \$24.95.

This book adds to the literature on the New Deal and the culture of the 1930s. Written by an admiring stepdaughter, the biography traces the career of Hallie Flanagan, who went from Grinnell College and then motherhood to the Vassar College Experimental Theater Program during the 1920s. A woman of extraordinary energy and intellectual curiosity, Flanagan became the director of the Federal Theater Program under the New Deal. The program employed out-of-work artists in dance, theater, puppetry, and circus that were meant to educate as well as entertain a national audience. Flanagan hoped that her efforts would also establish a relevant national theater, rooted in regional traditions, that would outlast the crisis.

Joanne Bentley uses numerous interviews with Flanagan's coworkers as well as many letters, diaries, and personal papers to construct both Flanagan's public accomplishments and her attempts to fuse her public duties with her private roles of wife and mother. She achieved that fusion only with the aid of a second husband who was content to play the private and supportive role usually reserved for women. Bentley is successful in presenting a woman at the center of one of the most important cultural and political institutions of the depression era. Flanagan helped shape the Federal Theater Program and fought to keep it alive through the difficult years of backbiting and bureaucratic warfare that threatened to kill it and that by 1939 ultimately did. Flanagan takes center stage in those endeavors, battling for her vision of the “Living Newspapers,” for plays such as *Power*, *One Third of a Nation*, and *The Cradle Will Rock*, all of which used montages of drama, cinema, music, and documentary to discuss sit-down strikes, utility power, and decrepit housing as part of a deteriorating social and economic system.

Although not a Communist, Flanagan was part of the Popular Front, which sought to regenerate American culture along more democratic and cooperative lines. In that effort, the Federal Theater Program had to fight censorship by local Works Progress Administration administrators, Congress, and, at times, New Deal allies. In New York, the program's most vital theater center, Flanagan also battled the Worker's Alliance, a radical group that considered the program a relief organization and the basis for political organizing. Until Martin Dies, the House Committee on Un-American Activities, and Franklin D. Roosevelt's conservative

enemies killed the program, Flanagan traveled and worked tirelessly on its behalf.

As Bentley admits, others will need to analyze more deeply Flanagan's personal demons and the political, social, and cultural content of her theatrical productions. In addition, although Bentley criticizes the Worker's Alliance for its politics, her focus on Flanagan makes for a one-sided presentation of their disputes. Yet the book provides important insights into the theater program and the New Deal. Certainly, the book challenges the view of the New Deal as merely a savior of capitalism, reminding readers of the political possibilities contained in the Popular Front and the New Deal. Can anyone imagine a Federal Theater Program of such magnitude and concern under any other president after Roosevelt?

LEWIS A. ERENBERG
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MICHAEL V. NAMORATO. *Rexford G. Tugwell: A Biography*. New York: Praeger. 1988. Pp. x, 192. \$39.95.

In *The Light of Other Days* (1962), Rexford G. Tugwell wrote about his boyhood, ending with his completion of high school in 1911. In *To the Lesser Heights of Morningside* (1982), Tugwell recounted his life in the years from 1911 to 1932, during which he attended the University of Pennsylvania, received his doctorate in economics in 1922, and taught at Columbia University from 1920 to 1932. In several volumes, including *The Stricken Land* (1947), *The Brains Trust* (1968), and *Roosevelt's Revolution* (1977), Tugwell wrote about his experiences as a presidential adviser and official in the Department of Agriculture and the Resettlement Administration from 1932 to 1936 and as governor of Puerto Rico from 1941 to 1946.

Autobiography is a genre with a journal and a body of critical methodological literature, but historians are seldom willing to leave to historical actors themselves the telling of their life stories. Paul Conkin's *Tomorrow a New World* (1957), my own *Rexford Tugwell and the New Deal* (1964), Charles T. Goodsell's *Administration of a Revolution* (1968), and other works complement Tugwell's writings. Still there is a need for a brief biography that could be valuable for comprehensive coverage (including treatment of Tugwell's academic career from 1946 until his death in 1979 at the age of eighty-eight), the exegesis of his ideas, and an assessment of his contributions as thinker and administrator to the development of the nation.

Michael V. Namorato's biography is disappointing. Early on he tells us, "The Tugwell that follows is not one who 'cerebrated' but one who acted" (p. 7). Namorato nevertheless devotes considerable attention, although with shortcomings, to Tugwell's thought. He sometimes fails to spell out concepts such as the "normalcy system" (p. 43). He states that the "Brandeisians, such as Tom Corcoran and Ben Cohen" (p. 116), or "the Frankfurter-Corcoran-Cohen group" (p. 128) or

the "Cohen-Corcoran group" (p. 170) were opposed to planning, but he does not identify their antitrust heritage derived from Woodrow Wilson and the New Freedom as opposed to the concentration-and-control tradition of Theodore Roosevelt and the New Nationalism within which, Otis L. Graham, Jr., has informed us, "Rex Tugwell lived his entire intellectual life."

Defects in content are unrelieved by felicity in form. I have identified twenty-eight dangling phrases and fifteen cases of indefinite antecedents. Countless times Namorato uses the last sentence of a paragraph to introduce the next paragraph. He sometimes states what goes without saying: "Everyone is influenced by someone or something; Tugwell was no exception" (p. 23); "Tugwell soon learned that what one wants is not necessarily what others are willing to give" (p. 31); "it is one thing to say someone influenced you; it is something else to recognize just how that person affected you" (p. 38); "academics do research and are expected to publish the results of their efforts. Tugwell did just that after 1946" (p. 153); "Tugwell was never, nor could he be, an ivory tower academic with no relationship to the real world. The world was his place to be and to work. Thus, he was active throughout his long life" (p. 169). In the next (confusing) sentence, Namorato intermingles Tugwell's administrative positions with "academic, lecturer, writer. Tugwell in other words was a doer."

For all its defects, including repetitiveness and occasional lack of clarity, students, properly cautioned, can read this book for a brief survey of Tugwell's life. Serious students of Tugwell's thought, the New Deal, and the history of planning in the United States will, despite Namorato's extensive research and documentation (32 pages of notes for 139 pages of text), turn to other works. Tugwell deserves better, and one wonders who, if anyone, edited Namorato's manuscript.

BERNARD STERNESHER
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CHARLES E. WYNES. *Charles Richard Drew: The Man and the Myth*. (Blacks in the New World.) Urbana: University of Illinois Press. 1988. Pp. xvi, 132. \$19.95.

Though not a familiar name to most white Americans, Charles Richard Drew is an important figure in medical history and almost legendary in Afro-American history. He is often (inaccurately) known as the man who introduced and perfected blood transfusions. That and other myths have arisen around this man who died young (age forty-five) and recently (1950).

Drew grew up in Washington, D.C., lettered in four sports at the exclusive, black Dunbar High School, and earned a respectable classroom record while excelling in varsity athletics at Amherst College. He attended McGill Medical School in Montreal, then trained in surgery at Howard and Columbia. As a research fellow at Columbia, he and his mentor John Scudder did important studies on banked blood and blood plasma.

Drew returned to Howard as a surgery faculty member in 1941 and remained there for the duration of his short, illustrious career.

Drew wished to be remembered primarily as a teacher of black surgeons but also for his groundbreaking 1941 report on the use of blood plasma for transfusions and for his leadership of the short-lived but extremely important Blood for Britain program at the start of World War II. Drew is better remembered, however, for things that he did not do but that were later ascribed to him: developing blood transfusions, publicly condemning the U.S. armed forces blood program for segregating blood by race, and dying because a Burlington, North Carolina, "whites only" hospital refused to treat him after an automobile accident. It is these and other myths that Charles E. Wynes describes and dispels in this first scholarly biography of Drew.

Using primary sources, including the Drew papers at Howard and interviews with Drew's family, friends, and colleagues, Wynes has written, by his own admission, "more of a biographical interpretation than a traditional biography" (p. xi). Aimed at a general audience, this book is the skeleton of a full-scale biography. Departing from the usual format of books in August Meier's "Blacks in the New World" series, Wynes is selective in the depth of his discussions, informal in his writing style, and conversational in his presentation of ideas and opinions. Chapters are short in length and thin on detail; for example, the sections on black and white medical education are oversimplified and in a few places inaccurate. Wynes's book does, however, make a real contribution to the fields of medical and Afro-American history, both because it is the first biography of Drew and because it convincingly dispels the pervasive myths surrounding this important black physician's life and death.

TODD L. SAVITT
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FREDERICK W. MARKS III. *Wind over Sand: The Diplomacy of Franklin Roosevelt*. Athens: University of Georgia Press. 1988. Pp. xiv, 462. \$29.95.

A book is the sum of its parts, and in that regard this book fails to add up. A reassessment of a major historical figure cannot rest, as this one does, on so selective a view. To label Franklin D. Roosevelt's diplomacy nothing more than a consistent, unmitigated failure defies logic and common sense, not to mention the evidence. The remarkable wartime coalition that made victory possible required diplomacy, not just a common enemy. Moreover, the economic and political goals of Roosevelt and his closest advisors were, in good measure, achieved during the war. Frederick W. Mark III's critique centers on Roosevelt's failure to avoid or prevent (it is not clear which) war with Japan, his long-term efforts as a "covert agent of appeasement" (p. xi) toward Germany, and his overall naiveté

and "parochialism" (pp. xii, 276). This is a sort of neo-neoconservatism, an argument that, if Roosevelt had avoided isolationism and provided firm leadership in Europe against appeasement, while simultaneously negotiating with the Japanese to avoid an unnecessary war, then, lo and behold, his foreign policy would have been realistic. Even the postwar world might have been different if Roosevelt had not been so naive and had stood up to Joseph Stalin, supported Chiang Kai-shek, and followed the example set by Winston Churchill in Greece. "One cannot resist speculating," writes Marks, "on what the results might have been if Churchill had administered Allied aid to Chungking and Roosevelt had directed the Western movement in Athens" (p. 193). Since the implied conclusion is that China would have been "saved" and Greece "lost," one cannot resist speculating whether Marks absorbed the evidence and arguments of Christopher Thorne and Lawrence Wittner, along with all of the other works cited in his impressive bibliography. The weight of the notes (one-quarter of the total pages) does not equate to the weight of the argument.

Marks justly criticizes the tendency to confuse Roosevelt's public and self-created image with the reality of policies, and he makes the important point that Roosevelt did set U.S. foreign policy during his presidency. Dismissing the notion that the president flew by the seat of his pants in foreign affairs, Marks points out that only by linking together diverse data can one understand "the full range of Roosevelt's foreign policy" (p. ix). Marks's argument makes sense only when modified by the understanding that policy could be made in the White House, but, during the Roosevelt years, the president could not ensure that his policies were consistently implemented. One wonders how differently things would have turned out had Roosevelt had at his disposal today's instruments of executive power in the field of foreign affairs.

Marks's insights are further flawed by what seems an overeagerness to say something—anything—new, as well as his constant Roosevelt-bashing. Perhaps Charles Tansill and Charles Beard would have agreed on the latter, but they were hardly dispassionate scholars on Roosevelt. Iconoclasm is always healthy, but not as a lawyer's brief disguised as scholarship. To pick but one example, Sumner Welles's mission to Europe in 1940 needs study and restudy, just as Marks claims, for it surely shook the British to the core. But to assert that the mission was "an outgrowth of FDR's seven-year exercise in appeasement" (p. xi) is both polemical and simplistic. So are the author's breathless claims that there are "remarkable gaps in the literature" (p. 3); that "much of the factual evidence presented appears for the first time" (p. 4)—a dubious claim once the word "important" is added; and that, unlike previous historians, he shows that Roosevelt during his first year in office "was exceedingly active on the diplomatic front" (p. x).

The logic and narrative of the book are difficult if not impossible to follow. As essays, some of the chap-

ters are useful for those with enough expertise to know they are getting a jaundiced, overdrawn view. But that monochromatic interpretation is too overpowering to ignore. Perhaps a single example will serve as surrogate for all the rest. According to Marks, "FDR has been called a cosmopolite, but to the degree that this may be true he was eclipsed by most of the foreign leaders with whom he had to deal" (p. 252). History would likely give that argument as regards Churchill and perhaps even Charles de Gaulle. But the examples that follow of more sophisticated leaders, offered by Marks without further comment, are howlers: "Chiang, who drew upon the academic tradition of three countries, studied in Japan and Russia, married the graduate of an American college, and embraced a Western religion. Stalin began by steeping himself in the kaleidoscopic variety of Soviet folk culture and, as agent for his party, attending international congresses in London and Stockholm. He toured Tammerfors, Cracow, and Vienna" (p. 252).

The author's archival research is extensive, his bibliography and endnotes are quite useful, and there are occasional points and arguments worth pondering. But this set of essays is of value only when used within the full context of scholarship on the Roosevelt foreign policy, a context that Marks has cited but not included.

WARREN F. KIMBALL
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DAVID F. SCHMITZ. *The United States and Fascist Italy, 1922-1940*. Chapel Hill: University of North Carolina Press. 1988. Pp. x, 273. \$29.95.

This book offers a narrative of U.S. policy making and diplomacy toward Italy from the Woodrow Wilson administration until June 1940. The author says that U.S. policy aimed mainly to stop Bolshevism and promote an international economy based on the Open Door principle. With few exceptions, he argues, policy makers were too blind or reactionary to recognize the true nature of Fascism. In their response to Mussolini, "U.S. leaders developed for the first time the rationale and logic for actively supporting right wing dictatorship in the twentieth century" (p. 4); moreover, "the significance of the prior experience with Fascism in Italy for the [Franklin Roosevelt] administration's policies toward Nazi Germany and the growing crisis in Europe during the 1930s cannot be overstated" (p. 5). When U.S. officials applied their mistaken view of Fascism to the Nazis, the wishful thinking and economic appeasement of the 1930s were born.

Richard Washburn Child, Thomas Lamont, Breckinridge Long, William Phillips, and many others undoubtedly deserve some of the harsh treatment they receive in this book. On the other hand, their views on Fascism have been known for many years, and David F. Schmitz is too preoccupied with their alleged mistaken analysis of Fascism to be able to provide a balanced

account of U.S. policy. His own grasp of Italian events and of the nature of Fascism seems shaky. For example, he mistakenly implies the existence of "liberal and Social Democratic masses" (p. 4) in Italy whom a more enlightened America (represented by Wilson) might have supported; he also implies that the lira was devalued (p. 109) during the stabilization of 1926. Of Fascism itself, Schmitz refers to its "basic requirements . . . as a social system" (p. 190) and says that the dictators' "power rested on their militaristic social systems and need to expand to maintain their power" (p. 192). This is rather thin as an explanation of Italian foreign policy in the 1930s. One wonders why the author did not consult basic Italian sources, for example, Renzo De Felice's *Mussolini il Duce* (1981), and Gian Giacomo Migone's *Gli Stati Uniti e il Fascismo* (1980).

The author says his book is about U.S., not Italian, policy (p. 2). But can one fairly assess the wisdom of Roosevelt's view of Italian foreign policy without a thorough understanding of one's own? Did Mussolini really go to war in June 1940 because of "the requirements of Fascism as a social system" (p. 190)? Given the opposition to war of people such as Foreign Minister Galeazzo Ciano, as well as the military and much of the Italian public, was Roosevelt really, as the author suggests, naive or misguided to try to split Italy from Germany? In his zeal to pillory U.S. officials, the author ignores the domestic political context in which they operated. He also says "no [Roosevelt administration] officials opposed Fascism as an ideology" (p. 170). What about Harold Ickes, Cordell Hull, Henry Morgenthau, and Roosevelt himself?

Schmitz's point about a link between the State Department's view of Fascism and its later German policy is plausible but remains to be substantiated. In any case, it is not very clear from this or other accounts what "economic appeasement" consisted of aside from talk. Finally, to illustrate his point about U.S. policy toward Fascism as a model for the future, the author says that U.S. recognition of the government of Pietro Badoglio indicated that "again it was the policy of the United States to trust that its long term objectives could be achieved by working with a right wing dictatorship" (p. 216). This statement suggests a rather limited understanding of U.S. policy toward Italy after 1943.

JOHN L. HARPER
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STEVEN T. ROSS. *American War Plans, 1945-1950*. (Garland Reference Library of the Humanities, number 845.) New York: Garland. 1988. Pp. xii, 167. \$25.00.

Nothing in this slender volume explains the author's credentials for a study of this kind, but those familiar with Steven T. Ross's survey of early modern French history for Garland's military bibliography series and

his editorial work on the French revolution for Krieger's European problems series may feel reassured that the work undertaken here is in good hands.

The U.S. Joint Chiefs of Staff (JCS), created in 1942 by the White House to institutionalize Anglo-American strategic planning, acquired in 1947 a statutory life of its own that endures to this day. In the five years analyzed here, the nation's top military strategists turned away from rosy anticipation of postwar Soviet cooperation to a bitter and anxious conviction that the Kremlin's ambitions endangered American values and, possibly, security. Whereas in 1939 six of the world's seven major powers were basically European, the post-war world had only two "super" powers, only one of them European. A heavily armed but badly injured Bolshevik empire towered over a Western Europe now demoralized and flattened by the rigors of fascist domination and the Anglo-American liberation. Only the United States, it seemed, could prevent Moscow's conquest of the "world island."

What response did our service chiefs propose for this largely unforeseen threat? The postulated enemy stood powerfully mobilized near the scenes of prospective action, while the United States was demobilizing. Lacking political guidance from the Truman administration, the JCS had to assume the worst and plan from there. The Pentagon nourished doubts that the Soviet Union would deliberately launch all-out war, but, if an accidental "misunderstanding" should trigger hostilities, there seemed little doubt that the war would become total with world dominion at stake.

Following their predecessors' thinking, the Joint Chiefs viewed a prospective "War Three" as a replay of the last world conflict, with an Anglo-American alliance struggling again to rescue occupied Europe and Asia. Favoring this approach, which led to regional defense arrangements around the Soviet periphery, were assumptions that British power would help to hold "Suez," North Africa, and India; the armies of Kuomintang China would assure Asia's redemption; and the U.S. Strategic Air Command's nuclear monopoly would overtrump the Reds' "conventional" superiority. In the period Ross surveys, all of these calculations fell apart, and JCS planning moved increasingly toward defending, not liberating, Western Europe with North Atlantic Treaty Organization formations buttressed by a firm U.S. military presence and a reborn German army.

Readers may wish Ross had devoted time and space to examining and identifying military and civilian scholarly input for many JCS calculations, for example, calculations that regarded the nuclear weapon as just a "more effective iron bomb" (p. 155) and saw a Soviet strategic asset in U.S. liberals continuing to nurture a generation-old hope that the Bolsheviks' conduct would ultimately match their vocabulary. Ross warns us that his concern here is with JCS products, not its "processes." We will have to be content for now with

this valuable treatise on U.S. military thought in a desperate hour.

LESLIE ANDERS

Central Missouri State University

MICHAEL M. BOLL. *National Security Planning: Roosevelt through Reagan*. Lexington: University Press of Kentucky. 1988. Pp. xi, 271. \$26.00.

The title of this work is somewhat misleading in that it suggests a study of the defense policy planning process. The book is, rather, a review of foreign and defense policy planning assumptions over the past half century. Michael M. Boll, a former foreign service officer with experience in Pentagon policy planning, identifies three elements of national security planning: consensus on national values established in national elections, identification of external goals and threats, and selection of instruments for attaining objectives.

A basic assumption informing this study is that a "high degree of accord" (p. 5) has existed in the post-World War II period over goals but that agreement about national security threats and protective instruments has often been lacking. In other words, the "great debates" over foreign and defense policy have been less about values and preferred outcomes and more about perceptions of threat and the choice of instruments for meeting threats.

Boll's analytical scheme for describing American foreign policy since World War II involves consideration of five major factors. First is threat perception. What are the capabilities and intentions of the adversary, primarily the Soviet Union? Second is strategic planning. How can the nation attain objectives in the most efficient way? His third factor is tactics. What specific policy options should be pursued? Fourth are arms control issues. Can the U.S. and the Soviet Union discover a common ground for limiting the upward spiral of mass destruction weapons? A final element in the author's analytical scheme is the institutional arrangement for national security decisions.

Pursuing these questions, the author describes events chronologically from World War II on. Chapter by chapter, he treats World War II planning, the origins of the cold war and containment policy, the emergence of institutional arrangements in the White House for national security planning, strategic planning under President Harry Truman and succeeding presidents, including efforts at arms control, limited wars in Korea and Vietnam, attempts at détente, the evolution of deterrence doctrine, the evolution of Marxist doctrine as it affected Soviet capabilities and intentions, and, finally, the planning assumptions of the Reagan years, which included a return to earlier aggressive cold war policies.

This book is primarily a review of U.S. foreign policy, emphasizing the goals, informational (intelligence) assumptions, and strategic military choices made in each period. The work is long on describing

the evolution of policy assumptions and on strategic generalizations and somewhat short on penetrating critical analyses of the choices made. Judgments are left to the reader.

The main value of the book is as a catalog of the principal foreign and defense policy events since World War II. The emphasis is on military policy as derived from assumptions about evolving Soviet capabilities and intentions. The author stresses the difficulties of delineating Marxist doctrine in the context of changing Soviet interpretations of strategic interests.

The book contains a useful bibliographical essay and copious endnotes demonstrating that the author has covered a vast literature on the period. Few archival or original sources have been consulted. The work, therefore, has the flavor of a very concise textbook of U.S. diplomatic, and especially military, policy over the past half-century. It will be useful to those wishing a concise survey, representing neither a "revisionist" nor an "antirevisionist" theme.

HARRY HOWE RANSOM
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MELANIE BILLINGS-YUN. *Decision against War: Eisenhower and Dien Bien Phu, 1954*. (Contemporary American History Series.) New York: Columbia University Press. 1988. Pp. xiv, 199. \$25.00.

In early 1954, the French were in trouble in Vietnam. The government of Prime Minister Joseph Laniel was spending millions of dollars each month to hold on to the colony with little to show for it save a long list of casualties and an increasingly strained relationship with its allies. Seeking a decisive victory over the Vietminh prior to the opening of the Geneva Conference on Far Eastern Problems in April, the French air-dropped twelve thousand troops into the valley of Dien Bien Phu and dared their enemy to fight a "set piece" battle. The Vietminh took the dare, and by late March the French garrison was in serious trouble. The Laniel government swallowed its pride and requested U.S. air strikes against Vietminh positions around Dien Bien Phu.

Melanie Billings-Yun shows that several key figures in the administration of Dwight D. Eisenhower wanted to grant the French request. The most zealous proponent of air strikes was Admiral Arthur Radford, chairman of the Joint Chiefs of Staff. Radford was joined, at various moments in the drama, by Vice President Richard Nixon, Harold Stassen, and, briefly, Secretary of State John Foster Dulles. All argued that U.S. strategic and economic interests in Southeast Asia would be jeopardized if France left Vietnam and that the fall of Dien Bien Phu would hasten French departure.

Through it all, Eisenhower alone remained consistent in his opposition to U.S. military action in Vietnam. Billings-Yun depicts Eisenhower as a president who understood the limits of power and appreciated

the horrors of war. Worried about his party's influential right wing, Eisenhower devised a public posture that suggested he would go all out in Vietnam, but he acted with restraint when the crucial decisions were made, out of the public eye. He deferred, postponed, muddled, and prevaricated to avoid making what he thought was an unwise decision. He made intervention conditional on British and Commonwealth participation, French willingness to move decisively toward independence for the Indochinese states, and the prior approval of Congress, yet he knew that the British were unenthusiastic about military action, that the French had refused for years to take seriously Indochinese demands for independence, and that key congressional leaders had already shown reluctance to use force in Vietnam. When advisers privately urged intervention, Eisenhower dismissed their arguments. In the end the conditions were not met. The president had tried, or so it seemed. In reality the president had rigged the process to fail.

This interpretation of Eisenhower's role in the Dien Bien Phu crisis is not new. Billings-Yun's conclusions about Dulles's position are rather more surprising. The secretary, she demonstrates, did not for the most part favor U.S. military action to rescue Dien Bien Phu. Dulles claimed that the French overestimated the significance of the battle: even if the garrison fell, Vietnam could be saved from communism. This was a critical objective, so Dulles doggedly pursued an allied commitment to guarantee that Indochina and Southeast Asia would remain in the Western camp after Dien Bien Phu. British disinterest in the scheme discouraged Dulles, and for a short time, during late April 1954, the secretary endorsed immediate U.S. air strikes. Ultimately, however, he agreed with his chief, and he made no effort to obstruct Eisenhower's plan to keep the nation out of war.

This book is the definitive study of U.S. policy during Dien Bien Phu, and that is surely what Billings-Yun and Columbia University Press intended the volume to be. It would be unfortunate, though, if scholars regarded the book as only filling a gap, for it is bigger and better than that. In addition to setting the record straight and offering a persuasive interpretation of Eisenhower's actions, Billings-Yun provides assessments of Dulles, Radford, Matthew Ridgway, and others that are concise and impressive. More than anyone else, she has studied the memoirs of French officials involved in Indochina policy making and placed their recollections alongside the more extensive American record, and she has articulated and resolved contradictions between the sources.

There are a few problems with the study. If the book is printed in paperback, I hope a proofreader will clean up the astonishing number of typographical errors. I hope, too (without much confidence), that the editors will allow Billings-Yun to provide more information in her skimpy footnotes. Of greater consequence is Billings-Yun's excessive admiration for Eisenhower. Revisionists have clearly captured him and borne him

triumphantly into the house of the holy. They have transformed uncertainty, inconsistency, and even stupidity into the calculations of a wise and wily man. This goes too far. Eisenhower was good, but he was not *that* good.

ANDREW J. ROTTER
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JAMES A. BILL. *The Eagle and the Lion: The Tragedy of American-Iranian Relations*. New Haven: Yale University Press. 1988. Pp. xiii, 520. \$25.00.

Ambitious in scope and successful in execution, this volume is both a history of U.S.-Iranian relations and a study of how Iranian policy was formulated in Washington. The final chapters sharply expose the institutional problems of how foreign policy is made with the Iranian debacle used as perhaps the ultimate example of how the system can produce failed policy.

The U.S. diplomatic failure in Iran dated from the 1950s, James A. Bill asserts, when Washington totally and uncritically committed itself to Reza Shah Pahlavi's regime. The United States cut off lines of communication with opposing elements, refused even to contemplate an Iranian future without the shah, and developed a tolerance for the shah's corruption, repression, over-reliance on militarization, and general incompetence. When it became obvious that the regime was fatally flawed and therefore doomed, the United States had been close to the shah for so long that its policy options were severely restricted.

This uncritical acceptance of the shah's rule on the part of U.S. government agencies, policy makers, the media, private citizens, and business leaders makes for disheartening reading. All too often the shah's supporters knew nothing about Iran, they were easily and effectively manipulated by him, and those who correctly raised doubts about American policy were either ignored or removed from positions of authority. A list of the shah's boosters resembles a roll call of the Washington establishment—among them, Lyndon Johnson, Richard Nixon, Gerald Ford, Jimmy Carter, Henry Kissinger, Zbigniew Brzezinski, Nelson and David Rockefeller, Senators Javitz and Jackson, Joseph Kraft, and Barbara Walters.

Their faith in the shah's invulnerability was badly misplaced. The lower class and most of the middle class were always on the outside of the shah's political system. The conservative, religiously oriented Iranian masses were deeply critical of Western intervention and influence. The shah's political support was limited to the landed aristocracy and the professional middle class. And by the 1960s his economic and social policies were badly failing.

The picture of the shah provided by Bill is of a terribly flawed political figure. One example of his excess and poor judgment was his decision to celebrate in 1971 what the shah insisted was the twenty-five hundredth anniversary of the Persian monarchy. He

linked himself with Cyrus the Great during the commemoration; in fact, the Pahlavi dynasty dated only from the shah's father, an unlettered military man who crowned himself king in 1925. In a nation with a per capita income of \$250, the shah spent \$200 million on the ceremony.

No president's political fate was more closely associated with Iran than was Jimmy Carter's. Bill conclusively refutes the conventional argument that the shah began to liberalize his political system largely in response to Carter's human rights policy. For a variety of domestic reasons, the shah had begun to liberalize months before Carter was elected. Carter's Iranian policies can be fairly viewed as a disastrous failure. By turns Carter was hypocritical, inconsistent, uninformed, and confused. If, indeed, it was the issue of Iran on which the American public evaluated and rejected Carter's presidency, it would appear that the administration got what it deserved.

For example, despite his public commitment to reduce U.S. arms sales, Carter quickly made an exception in the case of the shah, whose government purchased 50 percent of U.S. arms exports. Allowed to buy any conventional weapon and in any amount, the military and security establishment in Iran absorbed 40 percent of the government's budget. This overemphasis on militarization proved one of the regime's major economic roadblocks to success. Although Carter's Iranian diplomacy became a litany of errors and blunders, Bill believes that by the time of his presidency there was absolutely nothing that could have been done to save the shah. His early mistakes had been institutionalized.

Understandably, much of the book's focus is on the events since the mid-1960s. The one cautionary note concerning this book is that it was written too early for the author to have obtained documentation from official sources. Bill, an Iranian specialist, made a prodigious effort to collect the sources that were available. Because of his knowledge of the Farsi language, he successfully incorporated a wide selection of Iranian sources ignored by other writers. Nonetheless, until classified documents are opened, there will be unanswered questions about many issues. An example is the story of the 1980 mission to rescue the American hostages from their Iranian captors, which ended in tragedy as well as failure.

Of particular interest is Bill's examination of why U.S. policy toward Iran was so inept in its formulation. What went wrong in Iran was not the first time such mistakes were made, nor will it be the last. Bill demonstrates that the mistakes made in Iran are systemic to the institutions of diplomatic policy making. A foreign leader or government, for example, can influence U.S. foreign policy by building a network of influential supporters within this country.

The shah established a lobby of American supporters who easily ensured that State Department professionals and Iranian experts who urged a more critical stance toward the shah never influenced policy. The American public, for example, was fed a steady diet of

pro-shah stories by a carefully nurtured, uncritical media. Included in the nurturing were hundreds of gifts delivered by the shah to members of the media. The more favorable the coverage given the shah, the nicer the gift; Barbara Walters was sent caviar, Cartier silver, and a diamond watch following one of her interviews with him.

Many of the diplomats who were sent to Iran knew nothing of the country. Americans in Iran had little contact with Iranians, Americans lived well, and the Iranians whom they did know were from the upper classes. No U.S. ambassador to Iran spoke Farsi. Americans who shaped Iranian policy, either from inside or from outside the government, were all too often people who were misinformed about Iran, its history, and its culture. Unless changes are made in how policy is formulated, more disasters like the Iranian failure lie ahead in foreign policy.

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THOMAS BROWN. *JFK: History of an Image*. Bloomington: Indiana University Press. 1988. Pp. 150. \$19.95.

Twenty years after John F. Kennedy's assassination, a *Newsweek*-Gallup poll showed that he remained by far the most popular American president. Yet, during the same period, historians ceaselessly whittled away at his reputation and achievement. Meanwhile, biographical revelations exposed a tawdry side of his character, which also had virtually no impact on how the public continued to perceive him. Clearly frustrated by this gap between popular myth and reality, Thomas Brown has written a succinct and densely textured essay on the evolution of Kennedy's image since 1963, showing how and why his standing changed among scholars and writers even as it rose steadily in the popular mind.

As Brown sees it, Kennedy's reputation has gone through three major stages: first, the elegiac adulation that poured out after the initial shock of the assassination, beginning with the funeral itself and presided over by family members, friends, and loyal Kennedy retainers; then, the onslaught of revisionist attacks fueled by the disasters of the late 1960s and early 1970s, especially the war in Vietnam; and, finally, a more balanced, less debunking analysis, clearly shared by Brown, that avoids the excesses of the revisionists but scales down both Kennedy's achievement and our exaggerated view of the impact of presidential leadership and personality, instead subordinating Kennedy's role to that of "the attitudes, ideas, and institutions within which he functioned" (p. 83).

More than a bibliographical essay yet less than an independent account of the Kennedy years, Brown's book is in many ways an exemplary piece of work. As he shows us how the Kennedy image was created, virtually no facet of the legend escapes his attention: the endlessly repeated emphasis on Kennedy's style,

wit, and cosmopolitanism, as well as his cultivation of artists and intellectuals; the stress on his supposed pragmatism, realism, and nonideological brand of liberalism; an all-purpose appeal to personal growth as an explanation for the shortcomings of his early years and even his presidency. Yet, in his zeal to get behind the image, Brown sometimes falls into a debunking mood himself. Too often, instead of counteracting the Kennedy image with facts or interpretations of his own, he simply paraphrases it in ironic terms, as if every facet of the myth were self-evidently untrue.

Of the reputed "growth" of Robert Kennedy, for example, Brown writes: "He was, in the view of many of his admirers—and *certainly in his own eyes*—the heir apparent to the presidency, *destined* to fulfill the promise of his older brother. RFK, like JFK, *allegedly* had his phase of 'growth,' which was largely compressed into the period between JFK's death and his own, when he *ostensibly* matured and developed a personality independent of the fallen president's [emphasis added]" (p. 26). Proceeding by mild sarcasm rather than argument, such a heavy-handed passage is merely tendentious. (Revisionist arguments, on the other hand, Brown more often summarizes in neutral terms.) Whatever political calculations or personal demons fed Robert Kennedy's last years, the positions he developed on many issues, as well as the aura of concern he projected, were at least the functional equivalents of "growth." The special stature he achieved by 1968 was based on what he did, not what his admirers said about him.

Similarly, his brother's genuine achievements—the dramatic turnabout on civil rights, the American University speech looking beyond the cold war, the limited test-ban treaty—all belonged to the last six months of his truncated administration. These positive omens do not license us to indulge in counterfactual speculation about what either brother would have done—in Vietnam, say—had they lived, but these strong endings do suggest that at their deaths both men were becoming greater leaders if not better men. Here the logic of popular feeling is wiser than the sober yet partisan inquests of historians.

While Brown stresses that heroic images of the past are animated by new cultural circumstances and real human needs, he fails to acknowledge how legends can take on lives of their own. His book would have profited from some comparisons with the myths of Abraham Lincoln and Franklin Roosevelt, which were also developed around charismatic but controversial presidents who died suddenly in office. He should also have done more to explore the key role of the mass media in presidential politics since 1960, when Kennedy first packaged himself for a national audience.

Instead, Brown gets stuck on small details: that Kennedy had no real feeling for the arts, that poor blacks give Kennedy (rather than Lyndon Johnson) too much credit on civil rights, that the Kennedy ideal of youth and vigor was "intellectually vacuous" (p. 18). He is annoyed by the "malleability" (p. 106) of Kennedy's

image—look at how all four national candidates in 1988 could lay claim to it (as in the Bentsen-Quayle debate)—and by its resistance to rational correction. It is easy to dispose of the Camelot nonsense, yet the Kennedy administration was a turning point in the official recognition of the arts, and it did finally give great impetus to both the civil rights movement and the youthful rebellions of the 1960s. Although Johnson achieved far more on civil rights (partly by appealing to the Kennedy aura), blacks had every emotional reason to identify with a slain leader.

Despite its vagueness, which offers something for everyone, the Kennedy image has sometimes been closer to the truth than Brown allows. But even its fictional elements have had a potent influence. The myth itself has become part of our history, evoking a simpler, more hopeful era on which many Americans, liberals and conservatives alike, project their urgent wishes and needs.

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ELAINE TYLER MAY. *Homeward Bound: American Families in the Cold War Era*. New York: Basic Books. 1988. Pp. xi, 284. \$20.95.

"Why did postwar Americans turn to marriage and parenthood with such enthusiasm and commitment?" (pp. 5–6) is the central question underlying Elaine Tyler May's new book. May's opinion of this turn is clear, and "it might have been otherwise" (p. 8) is the refrain of this book. Baldly stated, the question seems to presage an attack on those who threw away the opportunity (created by the disruptions and dislocations of depression and war) for a new configuration of gender roles. Instead, May offers a sensitive, nuanced reading of domestic ideology, judging but never blaming. Her men are not oppressors, her women not betrayers. But neither are they simply victims of circumstance or of forces beyond their control. May allows her subjects agency; she insists, time and time again, that they make choices. She then carefully explores the institutional, cultural, and historical constraints that shape and limit their choices. History has a long—and often dark—shadow in this book.

May begins this work with a striking image: a young couple, just married, plan to spend their honeymoon in a bomb shelter. They appear in *Life*, surrounded by the canned goods and consumer items that will stock the shelter, kissing for the cameras as they descend into their secure retreat. This, May argues, "is a powerful image of the nuclear family in the nuclear age: isolated, sexually charged, cushioned by abundance, and protected against impending doom by the wonders of modern technology" (p. 3).

In this thoughtful and imaginative book, May makes clear the connections between the nuclear family and the nuclear age, between the cold war and the "domes-

tic revival" in postwar America. May looks to foreign policy, specifically containment, for what she argues is the central metaphor of the age. In fact, as May explains and demonstrates, containment is more than a metaphor: it is a strategy. According to contemporary political ideology, domestic stability and unity were crucial to waging the cold war, but modern American life threatened to undermine that stability. The home, it seemed, could contain the forces that might be disruptive—sex, affluence, women's aspirations—not only rendering those forces safe but also transforming them into sources of strength for the family and thus for America.

Although May consistently emphasizes the desire for security that propelled young Americans into the home (indeed, she sees security as the shared goal of foreign policy and domestic ideology), the people she describes are not simply fleeing the difficulties (foreign and domestic) of the postwar world, assuming a defensive posture in the suburban home/bomb shelter. "Rather than representing a retreat into private life," she argues, the new domesticity was "one way to express civic values" and to exert influence on the larger world (p. 136). If the family was the cornerstone of America's moral strength and thus, indirectly, of its security, then strengthening the family was a patriotic act.

Perhaps May's greatest success in this book is in showing the importance of prescription and cultural norms in shaping the meaning of individual experience. In addition to popular culture, demographic data, and the professional literature of various disciplines, May used the Kelly Longitudinal Study (KLS), a long-term psychological survey of three hundred (white, middle-class) married couples. The open-ended responses allowed in portions of the survey afford students of recent American history an unusual opportunity to hear the private voices of real people and to watch the interplay of convention and experience.

These men and women, in describing their lives, adopted the language of experts (the "psychological terminology popularized by professionals") and measured their "normality" against the Kinsey reports (p. 125). Most fascinating is May's discussion of the impact of sexual mores—"the sexual containment ethos," May calls it—on this group (p. 115). Although she offers nothing especially new in her description of the impact of "sexual containment" (psychologists and moralists proclaimed the dire consequences at the time), we find that the majority of men and women found their sexual adjustment in marriage largely determined by their reactions to the cultural imperatives against premarital sex. They, in general, reacted as the experts told them they would and called on such expertise to justify and explain their experiences.

There are some problems with this work. The initial image May offers us—the bomb shelter honeymoon as emblem of the cold war family—is striking but flawed. The postwar family was overwhelmingly child-centered. Those two weeks in a bomb shelter would have been a very different experience with three or four

small children. In fact, children are consistently slighted in May's analysis, figuring as decisions about procreation, sometimes as sources of pride, or as factors in their mothers' nervous breakdowns, but we get no real sense of the functioning or meaning of the family unit. May concentrates instead on the relations between husband and wife. Furthermore, her emphasis on the importance of sexuality in marriage (even if expectations were frustrated) seems overstated, judging by her presentation of the KLS data.

There are smaller quibbles. May's use of Hollywood as a barometer of popular culture is interesting but uneven, and frequent restatements of her thesis are sometimes intrusive, but the strengths of this book far outweigh any qualifications. May has made an important contribution to the current historical reassessment of the 1950s, has given voice to a much-maligned generation, and offers much to those of us—children of that era—who still seek the meanings of our parents' lives.

For all the poignance and eloquence of May's subjects as they reveal their hopes and dreams and disappointments, the words I will remember most are May's: "They made the choices they believed they had to make" (p. 207). It is a sympathetic picture, but still a grim one.

BETH L. BAILEY
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MARK SILK. *Spiritual Politics: Religion and America since World War II*. New York: Simon and Schuster. 1988. Pp. 206. \$19.95.

With this book Mark Silk has given general readers an engaging, well-written, and beguilingly brief chronicle of the role of organized religion in American society since the 1940s. His achievement in doing so should not be underestimated, for Silk manages to be learned at the same time that he is lucid.

This account is not church history as it is customarily rendered. Silk shuns the predictable features of the survey approach, such as an unstinting catalog of persons and their pronouncements, in favor of a kind of socially sensitive intellectual history in which distinct episodes that are nevertheless emblematic of America's special religious ethos are selected for discussion. That ethos, Silk believes, was constructed precariously atop a major fault line, a "tension . . . between exclusivist creeds," whose style he labels "conversion," and a "spiritually inclusive national faith," which he terms "adhesional" (p. 19). Americans, in other words, were ideally to value their particular religious commitments but not so much that those commitments submerged the common feelings that citizens were to share as Americans. As a result, however, any account of religion's place in American life is necessarily marked by a confusing oscillation between two apparently conflicting convictions, fused for all time (and for the continu-

ing instruction of generations of historians) in a now-infamous utterance in 1952 by president-elect Dwight D. Eisenhower. "Our form of government has no sense unless it is founded in a deeply felt religious faith," Eisenhower was supposed to have said, adding, "and I don't care what it is" (p. 40).

A cornerstone of this structure of thought, Silk argues, was the synthetic notion of a "Judeo-Christian tradition," a device that he claims was invented during the war to bind religiously diverse Americans together in opposition to the forces of fascism in Europe and to its sympathizers everywhere. "The idea," notes Silk, "was to invoke a common faith for a united democratic front" against totalitarianism worldwide, to create "the spirituality of the American Way" (pp. 41, 44). Religion, then, came to be regarded as the principal source of Western, democratic, American values, and the country's "adhesional" faith had its glue. But how, within such a scheme, could the specific—and often contradictory—appeals of multiple religious creeds be accommodated? Delicate ecumenical diplomacy, accomplished more frequently by gestures of indirection than by ones of integrity, was the task of the day.

In spite of a recurrent theoretical insistence, this is not a book that is wanting for either historical scope or colorful anecdotes. Within allusions to Roman Catholicism alone, Silk treats converts to the faith as varied as Clare Boothe Luce, Thomas Merton, and Avery Dulles; advocates of the church's relevance to modern conditions as dissimilar as Jacques Maritain and Bishop Fulton Sheen; contenders over the morality of war as different as Dorothy Day, Francis Cardinal Spellman, and Daniel Berrigan; and writers on religious liberty as opposite as Jesuits Leonard Feeney (he of the "Boston Heresy": *extra ecclesiam nulla salus*) and John Courtney Murray (who later served as architect of a momentous declaration on the subject by the Second Vatican Council).

Page for page, this volume condenses more authentic analytical depth and telling historical fact about the modern course of religion in the United States than almost any other volume currently for sale in trade bookstores, places where shelves dedicated to matters religious still sag under the weight of scriptures largely unread, personal testimonies gone unheeded, and inspirational tracts that border on the insipid.

KEVIN J. CHRISTIANO
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MICHAEL WOODIWISS. *Crime, Crusades, and Corruption: Prohibitions in the United States, 1900–1987*. Totowa, N.J.: Barnes and Noble. 1988. Pp. ix, 260. \$33.50.

As American as apple pie, moral crusades or prohibitions have been an integral aspect of modern American history. Generating tens of thousands of laws on the federal, state, and local levels, Americans have developed an active tradition of policing and regulating the social behavior of their neighbors. No wonder, then,

that during the twentieth century, as Michael Woodiwiss points out, the United States had the "most thorough oversight of personal behavior in the Western industrial world" (p. 1).

In this history of America's prohibitions, Woodiwiss analyzes the consequences of repeated attempts at moral suasion. Beginning with Prohibition, the grandparent of social legislation, and continuing well into the present with the efforts of the administrations of Ronald Reagan to combat the drug traffic, Woodiwiss explores the politics of social control and the process by which "virtue and abstinence became official state policy" (p. 5). Decidedly unsympathetic to these efforts, which he labels "excessive" (p. 8), Woodiwiss argues that such prohibitions have not only spawned an even more labyrinthian underworld but have led to the wholesale trampling of civil rights on the part of such zealous crime fighters as Thomas E. Dewey, once this country's premier "racket-buster."

Woodiwiss not only takes on Dewey and other sacred cows of the law enforcement community but also attacks the prevailing ideology of crime fighting among law enforcement officers in which the Mafia figures as the dominant criminal element. There is a "science fiction quality" (p. 228) about the alleged power and centrality of the Mafia within the underworld, he argues, suggesting instead that key personalities within the federal government deliberately invented a "scapegoat or conspiracy theory" of crime fighting; when it comes to understanding the etiology of criminal behavior, "the American people," he concludes dramatically, "have been the victims of a successful doublecross" (p. 229). In what is by far the strongest section of the book, Woodiwiss effectively traces the process by which the Mafia came to be linked inextricably in the public imagination with the underworld, a process that he believes originated with the Senate hearings chaired by Estes Kefauver in 1950-51 and then grew unchecked during the succeeding decades thanks to the encouragement of the popular press, the movies, and the law enforcement community. "By dulling the critical and analytical faculties of commentators and investigators," he writes, "the Mafia interpretation of organized crime became the conventional wisdom" (p. 7).

Although the book holds out the promise of a fresh perspective on a familiar topic, it falls short. The author's heavy-handed use and often-uncritical reading of secondary material, much of it highly tendentious to begin with, lead him all too often to make dramatically worded and broad statements that strain the credulity of the careful reader and oversimplify what is surely a more complex phenomenon than he depicts. Ultimately, the evidence Woodiwiss marshals in support of his claim that prohibitions have done more damage than good to the commonweal cannot sustain the weight of the interpretation he has placed on it.

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STEPHEN J. WHITFIELD. *A Death in the Delta: The Story of Emmett Till*. New York: Free Press of Macmillan. 1988. Pp. xiv, 193. \$19.95.

In this study Stephen J. Whitfield presents an account of the facts related to the murder of black teenager Emmett Till and attempts to assess the broader implications raised by this episode. We are given a portrayal of the known sequence of events: the encounter at the grocery store that triggered the impulse to violence, the shooting of Till by half brothers J. W. Milam and Roy Bryant, the dumping of Till's body into the Tallahatchie River with a cotton gin fan tied to the victim's neck, and then the farcical trial that predictably ended with the defendants' acquittal. Whitfield aptly captures the flavor of Mississippi racial justice and also conveys a sense of the courage displayed by Till's great-uncle, Moses Wright, who dared in open court to identify Milam and Bryant as the men who had seized his nephew.

The failure to punish the killers is appropriately related to the indifference of federal authorities. Willing captives of the complexities of existing law, Eisenhower administration cabinet officers had no taste for exploring avenues that might have opened the way for federal intervention. Federal Bureau of Investigation Director J. Edgar Hoover's response to the Till case, as Whitfield notes, was marked by "bureaucratic imperturbability and moral indifference" (p. 78). Hoover appeared less concerned with the horror of the Mississippi slaying than with the likelihood that Communists were involved in agitation for federal action. At several points the murder is thoughtfully related to the wider context of racial violence. Noting Oscar Handlin's "misleading description of the conditions of physical safety under which rural blacks lived in the South" (p. 101), the author observes that dozens of racially motivated murders and executions have occurred since 1954. Whitfield views changes in the pattern of lynching as connected to an increasing prevalence of "legal" lynching in which the forms of law were used to terrorize blacks. The point is driven home that "the decline in mob violence by the mid-century cannot convey the terror that permeated black communities in the South" (p. 8). In describing lynchings as communal acts, Whitfield discredits the notion that the respectable gentry generally opposed these atrocities. He also effectively challenges the view that terror had its source in lower-class resentments, although he does not take into account the possibility that members of the southern establishment may have been prime instigators of racial violence. The book provides a service in calling attention, as did Richard Hofstadter in one of his last works, to the lack of modern scholarly analysis of lynching. Our attention is also captured by information about the impact of Till's murder on various individuals, a number of whom appeared as prominent cultural figures and activists in the civil rights movement.

There are, however, several problems with this volume. Some of the conclusions are hastily considered

and unsubstantiated. There is carelessness concerning details. A master's thesis that used the trial record in discussing the case is itself described as a primary source concerning the slaying (p. 55); the murder of Malcolm X's father is mistakenly said to have occurred in Omaha, Nebraska, rather than in Lansing, Michigan; and the author generalizes with regard to the cases of Wayne Williams and Joan Little, while citing little in the way of supporting evidence. On the level of a conceptual approach to racist violence, Whitfield does not recognize the significance of white male aggression against black women as a source of lynching. He presents an unfocused defense of Susan Brownmiller's blurring of distinctions between Till and his murderers, expressed in her notion that they were all somehow linked in a common understanding of the aggression implicit in the boy's alleged sexual gestures. The author is unconvincing when he denies that Till's death was that of a martyr.

The wider ramifications of the murder are not fully grasped. Whitfield is excessively cautious in assessing the impact of this killing on the Montgomery movement, and this caution relates to a failure to assimilate into the book the extremely broad scope of the nationwide protest against the Mississippi brutality. That protest was a vital factor in establishing the context within which the bus boycott emerged. The episode is also isolated from its national setting when the author argues that "the Till murder needs to be understood as far more specifically a regional phenomenon" (p. 140). The violence took place in Mississippi, but it cannot be adequately understood if we do not take into account long-existing national policies that committed federal authorities to inaction against southern racism. Bertrand Russell may after all have been closer to the truth when he viewed the episode as proof of American intolerance and bigotry.

Whitfield's book movingly conveys some of the drama and tragedy inherent in the events, but we do not yet have the book that systematically explores the facts about the Till case and draws from them what can be learned about American patterns of racial violence.

HERBERT SHAPIRO
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IRWIN UNGER and DEBI UNGER. *Turning Point: 1968*. New York: Charles Scribner's Sons. 1988. Pp. viii, 568. \$24.95.

The tumultuous events of 1968 inspired Irwin Unger and Debi Unger to write this work, a history book with a thesis, namely, that 1968 was a crucial year that changed America in many ways. It was not only the year of assassinations, of the Tet offensive, of the Columbia University rebellion, and of the destruction of the government of Alexander Dubček in Czechoslovakia but also the year that marked the end of the old Democratic party coalition. It was the year in which American liberalism destroyed itself and the year in

which traditional conservative Americans embarked on the reconquest of political power that culminated in the presidency of Ronald Reagan.

The title of the Ungers's work is, however, somewhat misleading, because their book, instead of focusing narrowly on 1968, is, in reality, an abbreviated history of the entire decade. In seven chapters, they deal with the U.S. economy and the Great Society, the Vietnam War, the civil rights movement, Students for a Democratic Society (SDS) and the New Left, the hippies and the cultural revolt, and Democratic-Republican party politics.

As a history of the 1960s, the book is a very good summary of the period, set forth in thoroughly entertaining, journalistic prose. From a stance of tolerant, amused detachment, the authors describe the passionate politics of the era, but they make no pretense of ever having been personally involved in the history about which they write. And herein lies part of the dilemma of their book. For those who lived the history of the sixties, Todd Gitlin's *The Sixties: Years of Hope, Days of Rage* (1987) is an infinitely more satisfying book. For others, who view the sixties as simply a segment in the Democratic-Republican political continuum, the tale of liberalism's demise has already been told wonderfully well in Steven M. Gillon's *Politics and Vision: The ADA and American Liberalism, 1947-1985* (1987).

It would appear that nearly everyone who has dealt seriously with the decade has been tempted to view the year 1968 as a "raging vortex," or a sort of historical "Venturi tube" through which events were forced under pressure to emerge in different forms and shapes. But, as it turns out, history will not sustain the thesis except as it applies to electoral politics. The civil rights movement had long since abandoned nonviolence, and Martin Luther King's following was fragmented. His strength was spent. The American public could not bring itself to reject the carnage in Vietnam and demand immediate withdrawal as their acceptance of Richard Nixon's "Vietnamization" policy shows. Progressive labor had already infiltrated the SDS, and the SDS had already spread itself so thin, and wandered so far from the Port Huron Statement, that its own failure was easily predictable. The changes wrought in the cultural revolt were, in 1968, still incomplete and tenuous.

The Ungers clearly have not written their book from all printed and documentary sources available. All the same, without the burden of having to prove the remarkableness of the year 1968, the volume remains an excellent, brief survey of the major events of the 1960s. It is particularly strong in the area of economics, where many historical accounts are weak. The Ungers's analysis of the relationship between the catastrophic Vietnam War and America's relinquishment of world economic leadership is especially good. And yet, in their liberal objectivity and in their attachment to

mainstream politics, the Ungers miss the essential *Geist* of the decade.

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LAURENCE M. HAUPTMAN. *Formulating American Indian Policy in New York State, 1970–1986*. Albany: State University of New York Press. 1988. Pp. xiv, 215. Cloth \$49.50, paper \$16.95.

This is Laurence M. Hauptman's third successive book on the Iroquois and American government–Indian relations. His first two were *The Iroquois and the New Deal* (1981) and *The Iroquois Struggle for Survival: World War II to Red Power* (1986). This slim volume focuses on Iroquois relations with the state government of New York from 1970 to 1986. Hauptman investigates the neglect by the state of the Iroquois and their conditions, and he recommends a series of policy reforms to effect a statewide Indian policy. In brief, the author asserts that New York serves the rest of its population, while using resources from ceded Indian lands.

This case study began as a report prepared by the author in 1986 for the governor of New York and for American Indian nations. Expanded and revised for publication, it is a comprehensible description of the issues of the loss of Indian land, land claims, state criminal and civil jurisdictional problems, biased state services, chaotic federal and state responsibilities, conflicting state powers of taxation, tribal versus state sovereignty, exploitable Indian burial sites, and American Indian underemployment in the state government.

The plethora of data is the book's strength, but the difficult transition from report to book has weakened conceptual analysis. For instance, an account of Indian reaction to state policy actions and an exploration of the sociocultural dynamics between Indian communities and the state government would have enriched this study (as evidenced by the author's two earlier works). It is well known that the Iroquois are and have been among the most politically and culturally active Indian groups. Political relations, primarily New York's poor service of its Indian communities, are the thrust of this work. Graphically, three maps and six appendixes supply additional information covering population comparisons, sizes of reservations, tribal enrollments, health statistics, road development on reservations, and museum commentary on burial sites.

This case study serves as an example for future works regarding tribal-community developments. Too often, policy studies focus on national programs and federal-tribal cases, disregarding state-Indian interaction, which occurs on a daily basis. Policy makers and government officials would benefit from reading this

book, as would scholars and those readers interested in state-Indian relations.

DONALD L. FIXICO
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CANADA

HANA SAMEK. *The Blackfoot Confederacy, 1880–1920: A Comparative Study of Canadian and U.S. Indian Policy*. Albuquerque: University of New Mexico Press. 1987. Pp. xii, 236. \$27.50.

It is commonly asserted that Canadian Indian policy has consistently been superior to U.S. policy, in terms of both day-to-day administration and concern for native rights. In a comparative analysis of Canadian and U.S. relations with the confederated Blackfeet, Hana Samek questions many common assumptions while providing useful insights into the historical problems that confronted Indians and Indian policy makers on either side of the border.

Samek's study begins with a realistic assessment of the historical background of Indian policy in the United States and Canada. The differences between Canadian and U.S. policies, in Samek's view, stemmed from several related, historically rooted factors. First, the relatively slow growth of Canada's western frontier made rapid Indian dispossession less necessary north of the border, eliminating one of the key causes of the harsher policies in this regard in the United States. Second, in part because of the slow frontier expansion and in part because of the continuing prevalence of British attitudes toward law and order, Canadians in general and even Canadian frontiersmen were willing to comply with a significantly more authoritarian and centralized scheme of Indian administration than was possible in the United States. Finally, as a result of all of the above factors, Canadian policy makers tended to be more conservative in the formation and implementation of policy than were Americans. Although this occasionally resulted in disastrously slow adaptation to changing conditions, the general outcome was to minimize the disruptive effects of constantly fluctuating policies that were a natural consequence of the more progressive and experimental approach to policy making in the United States. In all, these have shaped the popular perception of Canadian policy as more enlightened than that of the United States, but Samek argues cogently and convincingly that these differences were the product of historical circumstances and not of greater sensitivity or wisdom on the Canadians' part.

In fact, despite popular misconceptions to the contrary, Canadians were apparently no more respectful of Indian sacred and secular culture than were Americans, and the author illustrates the many shared opinions that motivated white administrators in both countries. Samek reveals that the primary purpose in the policies of both nations emphasized the destruction of Indian culture and the "mainstreaming" of the Indian

population as the related notions of the "vanishing Indian" and of innate Indian savagery motivated idealists and pessimists alike to pursue a policy of Indian acculturation and absorption. In this effort, both nations failed equally, though for very different reasons.

This carefully researched and competently written book serves as an excellent and timely reminder of the importance of comparative study and of the ironies that invariably arise when considering the objectives and accomplishments of Indian policy. Although specialists may find much of the discussion concerning the intricacies of Canadian and U.S. Indian policies somewhat superficial, the comparative perspective more than compensates for the lack of depth. On the other hand, students of Indian policy, whether at the undergraduate or graduate level, will find this fresh reassessment of some seemingly timeless myths to be highly rewarding.

CHRISTOPHER L. MILLER
Pan American University

JOHN E. ZUCCHI. *Italians in Toronto: Development of a National Identity, 1875-1935*. (McGill-Queen's Studies in Ethnic History, number 3.) Kingston: McGill-Queen's University Press. 1988. Pp. xiv, 255. \$29.95.

Toronto today boasts the largest Little Italy in Canada. John E. Zucchi's book is a competent study of that community during its early years. Before 1940 Toronto's Italians lived in several neighborhoods and included both a transient population of unskilled laborers and a group of settlers with a broader range of occupational skills. Although he does not ignore the transient population, Zucchi focuses on the formation of a community among the growing population of permanent residents. He traces the gradual switch among the immigrants in Canada from village loyalties to a stronger sense of Italian national identity.

That theme is not a new one to scholars of Italians in the New World, but Zucchi explores new dimensions of it. He is especially effective in showing how occupational clusters (the products of chain migrations), Catholic and Protestant church leaders, and the ambitions of the immigrant elite contributed to the development of an Italian national identity in Canada. A chapter on how Benito Mussolini's representative exploited national sentiment to take over community institutions is one of the best accounts available of that unhappy period in the history of Little Italies in North America.

Like the authors of many recent studies, Zucchi portrays a community centered around business enterprises, church and ethnic organizations, and their leaders. Italian families and the ties of neighborhood and kinship do not get much systematic attention; nor do the tensions introduced by class divisions. Perhaps those topics receive sketchy treatment because national identity emerged in the institutions Zucchi describes rather than in family or class solidarities. One wishes, however, that Zucchi had explored that national iden-

tity with more analytical rigor, defining how it differs from what is more usually called ethnicity, which scholars have sometimes closely associated with either family solidarities or class consciousness.

Although he has written a community study, Zucchi insists that his book is "an example of immigration [or] migration history" (p. 4), which transcends the categories of national histories to focus on the immigrants themselves. As he has demonstrated in earlier papers on Italy's wandering artisans, Zucchi knows how to write migration history, employing a "village-outward" approach that follows groups of migrants around the globe and often back to their villages of origin. Yet that type of study is not what he offers in this book. Perhaps fearing that a community study might be read as one more contribution to Canada's ongoing discussion about ethnicity and ethnic group formation, Zucchi reflects too little on how his research contributes to the sizable literature on immigrant communities and ethnic leadership in North America. Because Zucchi offers few comparisons between Toronto's Little Italy and Italian communities elsewhere in North and South America, his book cannot transcend the concerns of national history, as he so obviously hoped it would.

DONNA GABACCIA
Mercy College

JAMES G. GREENLEE. *Sir Robert Falconer: A Biography*. Buffalo, N.Y.: University of Toronto Press. 1988. Pp. xii, 407. \$37.50.

If the colorless corporate manager is today's model for the successful university president, James Greenlee's biography of Sir Robert Falconer reminds us that this was not always the case. The book recalls an era when university presidents, like prominent politicians and trusted pastors, were paragons of social respectability, exerting a paternalistic influence on the communities they served.

The son of a Presbyterian missionary and a clergyman himself, Falconer assumed the presidency of the University of Toronto in 1907, a position he held until 1931. Trained in the classics, he guided Canada's largest university through a period of expansion and modernization, convinced that higher education could be both a force for moral order and a training ground for specialists and professionals. Like other Canadian educators in the early twentieth century who sought to bridge traditional values and current exigencies, Falconer believed that nonsectarian universities need not be godless institutions.

In elegant and persuasive prose, Greenlee explores Falconer's attempt to place his stamp on the university and the university's stamp on Canadian society. Within the institution Falconer restored administrative order, strengthened the graduate program, and kept interfering politicians at bay. With equal vigor he supported union government in 1917, campaigned for the union of the Presbyterian and Methodist churches, and pro-

moted Canada's role as an "interpreter" in Anglo-American relations.

Yet, in his effort to be all things to all people, there were contradictions and lapses. He was too often beholden to the business leaders who ran the university's board of governors, and he mishandled the reorganization of the medical school in the early 1920s. Although he objected to the participation of professors in politics, his own Conservative loyalties were never in question. He grappled, sometimes clumsily, with the issue of academic freedom, but he was not the iron-fisted demagogue caricatured by some of his critics.

Sympathetic without being sycophantic, this biography is an insightful account of a critical period in the development of a university whose history has yet to be written. If there is a gap, it is Greenlee's failure to discuss in any depth the president's association with student life and culture. Indeed, at times Falconer's public and political engagements preoccupy the author at the expense of a fuller exploration of the president's and the institution's academic activities.

Nonetheless, in situating Falconer on the intellectual landscape of Canadian society in the early twentieth century, the author makes some clear-headed observations. A "garden-variety idealist" (p. 155) who synthesized the thoughts of others, Falconer believed in the possibility of reconciling reason with faith, freedom with order, science with religion, general education with specialization, imperialism with continentalism, and progressivism with conservatism. Was he, alas, the classical middle-class Canadian? It is a label that Sir Robert, on the advice of his able biographer, would be unlikely to disown.

PAUL AXELROD
York University
North York, Ontario

ROBERT BOTHWELL. *Nucleus: The History of Atomic Energy of Canada Limited*. Buffalo, N.Y.: University of Toronto Press. 1988. Pp. xx, 524. \$34.95.

This study is official history in the grand manner. Robert Bothwell's account of the evolution of the Canadian national atomic energy agency occupies 450 pages, with extensive notes, and explains how Canadians parlayed a marginal involvement in the Allied wartime atomic bomb project into a sizable research operation that came to specialize in nuclear generation of electricity. Bothwell is the acknowledged specialist on Canada's participation in the atomic age, for he has already produced an equally sizable volume on the state uranium enterprise, *Eldorado, Canada's National Uranium Company* (1984). Thus, he is able to guide readers confidently through the intricacies of such things as reactors and piles, with the help of little boxes of type set off from the text to make key concepts clear to the scientifically subliterate like myself.

Possession of proven uranium reserves at a time when that strategic metal seemed in short supply

allowed the Canadians to persuade a British team of atomic researchers to locate in Canada during World War II. By the time peace returned, an independent laboratory had been established at Chalk River near Ottawa, which soon had an experimental reactor in operation. Because Canada lacked the elaborate plant to enrich uranium, development focused on a natural uranium reactor using heavy water as a moderator, although heavy water had to be obtained from the United States for a long time. By the mid-1950s, Atomic Energy of Canada Limited (AECL) had persuaded Ontario Hydro, the provincial electrical utility, to install the first of a series of these nuclear generators, and AECL soon embarked on efforts to market its CANDU reactor system around the world.

In a leisurely, discursive fashion, Bothwell guides the reader through the political, scientific, and technological thickets along this road using a wide range of manuscript sources and turning aside as he pleases to examine issues in detail. He does not, however, fall into the company historian's sin of trying to name everybody who was ever employed at AECL; the focus is firmly on the senior scientists and engineers who guided the company and their critical relationship with the politicians who paid bills. The narrative ends about 1970.

The lot of the official chronicler is not always an easy one, and in this book there is a sense of things unsaid and themes unexplored to their logical conclusion. The CANDU, in the face of fierce competition, never caught on outside Canada, although it seems to operate reliably and economically by comparison with rival technologies, turning out almost half of the power consumed in Ontario at present. Yet one gets the sense that Bothwell feels that the international marketing of reactor systems was doomed to fail from the start for a country with as little economic and political clout as Canada. Yet, without the promise of commercial success, public funding for development would never have been forthcoming. Nor is much attention given to the comparative costs of nuclear and hydrocarbon energy supplies, especially now that the bills are beginning to come in for disposing of the wastes that reactors produce. Future historians may well want to tell this story from another point of view, but Bothwell has given them a sound basis from which to commence their work.

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LATIN AMERICA

JUDITH LAIKIN ELKIN and GILBERT W. MERKX, editors. *The Jewish Presence in Latin America*. (Thematic Studies in Latin America.) Boston: Allen and Unwin. 1987. Pp. xi, 338. \$39.95.

This volume of essays, based on papers presented at the Research Conference on the Jewish Experience in

Latin America held in Albuquerque, New Mexico, in 1984, makes a substantial contribution to the relatively new area of the study of Jews in Latin America. Generally of high quality, the essays both provide us with a great deal of information about the uniqueness of the Latin American Jewish experience and place it in a world context. Readers interested in comparative aspects of Jewry will be well rewarded here. The four sections of the volume—"Historical Issues," "Comparative Dimensions," "Adaptation and Evolution," and "Defining a New Identity"—include nine general articles on Jews in Latin America, five on Argentina, two on Brazil, and one on Costa Rica. The authors bring a wide range of perspective and expertise to this very important topic.

A purpose of the original conference, and thus one of the most important focuses of these essays, is that knowledge of Latin America can be furthered through the study of its ethnic minorities and, concomitantly, that the status of Jewish citizens is of the utmost relevance to Latin America. In the initial essay of the volume, Gilbert W. Merkx points out two paradoxes that provide insight into the Jewish situation and into the nature of Latin American society. First, Jews have had relative success in Latin America and yet have been politically and ideologically marginalized; second, Jews have occupied a minor historical role and yet have been accorded an exaggerated importance in the contemporary myths of popular Latin American culture (p. 6). Most of the subsequent essays proceed within this framework.

Regarding the historical background of these issues, Anita Novinsky takes us further back into history than the authors of the other essays do in order to compare the Jewish situation in twentieth-century Brazil with that of converted Jews at the time of the Inquisition. Working with Inquisition records, Novinsky shows how Jews in Brazil were simultaneously integrated and marginalized. Her work also illustrates how the Jewish experience changed from region to region and in different historical periods, for example, in the seventeenth century in the period of Dutch control of northeast Brazil when the Jewish situation improved but when Jews were also accused of being traitors who had turned over Brazilian territory to heretics (p. 38).

According to Daniel C. Levy, one of the most important political factors that has influenced the Jewish community in contemporary Latin America has been authoritarianism, which appeared most prominently in the 1970s in countries holding four-fifths of the Latin American Jewish population, namely, Argentina, Brazil, Chile, and Uruguay (p. 160). Several of the essays deal with Jews as victims of these repressive regimes. Levy, in his work on education, looks at the degree of Jewish autonomy within the political constraints imposed by dominant groups and the state and by broader phenomena such as corporatism. Several essays reflect the fact that Argentina provides an especially apt country in which to examine the effect of authoritarian regimes on Jews. Bernard Segal, for

example, discusses Argentina's lack of an open political system to complement its history of social receptiveness to newcomers. In his work on Argentine right-wing nationalism, Carlos H. Waisman analyzes the ideology of the popular Argentine magazine *Cabildo*, which has blamed Jews for guerrilla activity (p. 242). On the other hand, Robert M. Levine furnishes a vivid analysis of Jewish cautiousness and conservatism based on questionnaires and interviews. He discusses the flight of Jews from leftist regimes such as those of Cuba and Nicaragua after 1959 and 1979, respectively, and from Salvador Allende's Chile in the early 1970s as well as the Jews' denial of Jacobo Timerman's allegations that the Argentine military was anti-Semitic (p. 76).

Space does not permit a discussion of the interesting material on education, both formal and informal; on Latin America's role in the history of the Zionist movement; on the similarities between the experience of Jews and that of other immigrants to Latin America; and on challenges to contemporary Latin American Jews, analyzed so well in the final essay by Judith Laikin Elkin.

KRISTIN H. RUGGIERO
Holy Cross College

ALAIN ROUQUIE. *The Military and the State in Latin America*. Translated by PAUL E. SIGMUND. Berkeley and Los Angeles: University of California Press. 1987. Pp. ix, 468. \$37.50.

The swings from civilian to military rule in Latin America frustrate analysts and confuse casual observers. Currently the tide favors elected governments, but military officers persist in being major actors in the region's politics. Not since Edwin Lieuwen's and John Johnson's books in the early 1960s have we had a study that examines the military's role in the entire region. This fine translation by Paul E. Sigmund of Alain Rouquié's *L'Etat militaire en Amérique Latine* (1982) examines the formation, evolution, and political functions of Latin American armies, while analyzing "the transformations of the state and the political system that military usurpation produces" (p. 10).

From a wide reading of the secondary literature, research in Argentina and Brazil, and his experience as French ambassador in El Salvador, Rouquié successfully offers a comparative perspective on the physiology of military power. Key to his explanation of the military's role in politics is the idea that there was a "time-lag between the modernization of the military and the modernization of the state" (p. 13). He observes that, "in all of South America, in the beginning was the army" (p. 43).

Yet, in the armies of independence, there were few professional officers. The development of armies and of Latin American states occurred in three stages. In the first, the *caudillos* of the postindependence era represented a "predatory militarism" (p. 43) without a professional military. They were an obstacle to the

formation of the state. The second stage saw states coalesce and national armies form but with their personnel coming from the ruling classes. In the third stage, early in this century, the military professionalized either parallel with or in advance of the modernization of the state. The armies served as both the core and the defenders of the state. Rouquié's chapter titled "Establishment of the Military and the Birth of the State" should be read by all those seeking to understand the Latin American military.

The military's technocratic and national mentality made officers intolerant of the clashes and compromises among regional, social, and economic interests that are basic to democratic, electoral government. They dreamed of a "depoliticized" state that would modernize the *Patria*. Their task was to defend not "the cause of the people . . . but . . . that of the state" (p. 286). Their goal was "to eliminate politics, not to found a new political order" (p. 269).

U.S. military officials saw Latin American armies as potential reserve forces in global struggles. The United States provided training and equipment with the intention of strengthening their reserve capability, which actually enhanced their ability to influence and to intervene. Latin Americans rarely obeyed passively Washington's commands. Rouquié hopes that the United States has learned that support of dictators simply because they are pro-American "is the best way to promote Communism" (p. 403).

Whether the present trend toward elected, civilian governments will last is still uncertain, especially because the military has "repudiated none of their past actions" (p. 404). The ghosts of the many thousands of "disappeared" throughout the region still moan for justice. The problem of getting armed men to obey unarmed sovereigns remains. As Rouquié observes in closing, "The old Latin adage *Quis custodiet ipsos custodes* [Who will guard the guards] has never been more true" (p. 404).

FRANK D. MCCANN
University of New Hampshire

JAMES DUNKERLEY. *Power in the Isthmus: A Political History of Modern Central America*. London: Verso. 1988. Pp. xvi, 691. \$47.50.

The Sandinista revolution and related crises since 1979 have stimulated considerable research and writing, but until now no one had successfully synthesized the plethora of publication and reporting into a comprehensive history of contemporary Central America. James Dunkerley's book is a most welcome volume, therefore, and is very nearly a definitive history of twentieth-century Central America. This exceptionally fine work reflects meticulous sifting through voluminous and disparate sources, both Central American and foreign, and effective organization into a thoroughly documented and refreshingly clear narrative. He is especially successful in the difficult task of com-

bing a regional approach with specific state-by-state analysis, as he emphasizes throughout the volume the conflicting themes in Central American history of unity and diversity, continuity and rupture.

The work embraces the history of the five Central American states from national independence through 1987 with increasing detail as Dunkerley approaches the present. A single, but excellent, chapter on the period from 1820 to 1910 opens the work, condensing much recent research and emphasizing the rise of the liberal, export-led development model for the entire region. In a second chapter, Dunkerley explains how the liberal oligarchy reached its apogee during the second and third decades of the present century. Here he convincingly analyzes the transition of the liberal coffee oligarchies to the more military-oriented dictatorships of the 1930s, emphasizing U.S. complicity in this transition. He does a particularly good job of explaining the reasons for specific variations in individual states from the general development pattern he describes for the isthmus. Two more chapters describe the changes that occurred in the oligarchies as they became a national bourgeoisie in the middle of the century (1930-80) and changed their methods of holding power. Although he continues to deal with all five countries as a region, with considerable comparison among them, a good part of these chapters becomes a series of separate accounts of the five states' experiences. These chapters offer a fine synthesis of the politics of all five states but are especially noteworthy on Costa Rica, El Salvador, and Guatemala.

In a separate chapter dealing with the region as a whole, Dunkerley concentrates on the uneven urban and economic growth between 1950 and 1980, providing considerable data on the changes in infrastructure, the maldistribution of wealth, and the continual faulty analysis of U.S. policy planners who blamed excess population rather than unequal income distribution for the region's problems. This chapter contains a wealth of specific information on the agriculture, exports, and urban sectors of each state.

The remaining six chapters—more than half of the book—focus on the individual states during the last fifteen years. Dunkerley begins with two long chapters on Nicaragua, one focusing on the origins of the Sandinista revolution and the other assessing Sandinismo in power. Single chapters on Guatemala, El Salvador, Honduras, and Costa Rica follow, but all of the chapters contain much comparative comment, so that the regional focus is never entirely lost. These chapters provide the best summary analysis presently available of the contemporary political situation in each Central American state, and they effectively relate recent events to the region's historical development. Dunkerley argues throughout that the revolutions in the region are deeply rooted in economic inequality and modernization policies that excessively favored elite and foreign interests. He is sharply critical of U.S. policy in the region, and he warns that the revolution in Central America is not over. "Little or nothing has been

done to alleviate the socioeconomic factors that lay beneath the revolts of 1979," and "in El Salvador and Guatemala exploitation and oppression remain unabated beneath a 'redemocratization' built upon fear and death" (p. 661).

Dunkerley's work on twentieth-century Central America ranks with Murdo MacLeod's *Spanish Central America* (1973) on the sixteenth and seventeenth centuries in providing a basic history that will become the standard for the period. Balanced and judicious in its interpretation, and loaded with factual data that will make it an essential reference work, the volume will serve both the beginning student and the serious scholar for many years.

RALPH LEE WOODWARD, JR.
Tulane University

PATRICIA SEED. *To Love, Honor, and Obey in Colonial Mexico: Conflicts over Marriage Choice, 1572-1821*. Stanford: Stanford University Press. 1988. Pp. viii, 322. \$39.50.

In a well-written revision of her dissertation, titled "Parents against Children," Patricia Seed describes the marriage practices of one group of colonial Mexicans. Using an uncertain number of cases (either 340, 389, or 705) in which parents or guardians attempted to overrule the marriage choices of their children, the author hopes to change our notions about the history of marriage that have emerged from historical scholarship during the last twenty years.

The author argues that in the years between 1574 and either 1689 or 1720 (the parameters are unclear), church and society combined to support the marriage preferences of men and women and to block the efforts of parents who attempted to impede the choices of couples. The procedures of the church, witnesses in the disputes, as well as the outcomes of the cases supported the decision of the partners. When parents opposed particular marriages, both the church and society asserted that love was the prime motive for marriage, and material interests and social inequalities—motives introduced by parents—should be disregarded.

Seed claims that, sometime between 1689 and 1720, society began to accord to parents the right to control the marriage choices of their offspring, an event that accompanied the rise of a "normative patriarchy which began to develop in the eighteenth century" (p. 91). The heart of the book is the exposition of the legal cases, and this material is presented in a largely anecdotal fashion. The quantitative material is presented in one brief chart, in percentages, and in figures included in the bibliographical essay. A troubling aspect of the work is that so little of the evidence is demonstrated in tables; figures are scattered throughout the text, notes, and bibliographical essay. The nature of the hypothesis should have been supported by tables and a more consistent quantitative dimension.

The hypothesis, however, does not depend on the

legal cases alone. It is bolstered by an exploration of the concepts of love, will, and honor as reflected in a selective reading of sixteenth- and seventeenth-century literature, legal tracts, dictionaries, and canon law in which these ideas and words are assumed to reflect social realities and changes. Seed's use of these ideas is disquieting, because it has not yet been proved that the literature of the Spanish Golden Age reflected social conditions. Literary conventions often governed plot and motivation, and language evolved in ways unrelated to social customs.

Some of the historical interpretation is also debatable. The interpretation of the decrees of the Council of Trent involves a possible conflation of the terms "choice" and "consent." The church promised only to support consent; initial marriage arrangements may still have been the prerogative of relatives. Another issue concerns the practice of parents regarding the inheritance of property; it is by no means certain that parents could or did deprive disobedient offspring of their share of familial wealth. The assumption that Mexicans had free choice in marriage in the seventeenth century, which came to be reversed in the eighteenth century, cannot yet be supported by the evidence presented here. Seed investigated a significant body of documentary material; it is regrettable that she has missed the opportunity to present the results of her other quantitative research. In the end, the book does not tell us about marriage choice or about the relations between parents and offspring regarding the choice of spouses. The book tells us about a certain number of cases of conflict. Although it has broadened our knowledge of the topic, I remain unconvinced of the validity of her argument.

EDITH COUTURIER
National Endowment for the Humanities

ALVIN MAGID. *Urban Nationalism: A Study of Political Development in Trinidad*. Gainesville: University of Florida Press. 1988. Pp. x, 294. \$25.00.

This book is a rather conventional political narrative of a short period in the history of Trinidad and Tobago. It begins with a rambling review of the late Spanish colonial experience and ends with a marginally relevant discussion of some of the mainly social science literature of the 1970s, especially works on the ideological concept of black power and the sociological concept of pluralism. The focus of the study, however, is on the period between 1895 and 1914. The selection of the dates stems from Alvin Magid's idiosyncrasy. From the local perspective, 1895 was not a particularly auspicious year in the history of Trinidad or Tobago (and the author exhausts five of his ten chapters before he reaches that date). It was the year Joseph Chamberlain took over as secretary of state for the colonies, and Magid presumes that, given Chamberlain's extensive background as mayor of Birmingham and his reformist interest in Parliament, he would have had a greater

impact on municipal life in the outposts of empire. In May 1914, however, the Colonial Office in London recommended, and the local Trinidad legislative council approved, an ordinance that established modified electoral status to the city council of Port-of-Spain, an event of marginal political significance. To a certain extent, then, this book is about the operation of imperial politics at the local level.

The complicated relations among the growing urban population, local colonial administrators, and central imperial authority are thickly described with a dependence on the official correspondence coloring the account. Much is told of the English governors and English colonial secretaries serving during the period, but little new information appears on the local participants in this unfolding challenge to British colonial administration. Despite the brave efforts of Magid to use the water riots in Port-of-Spain in 1903 as a sort of litmus test of crown colony government, nothing appears in this study to undermine the conviction that this era was an unexceptional period for the colony or the British empire.

Based on conventional sources, Magid's book provides neither new information nor new perspectives on the political development of Trinidad. The concept of urbanism is tortuously repeated but never satisfactorily explained, and the author himself appears to be ambivalent about the exact meaning of urban nationalism. Port-of-Spain, in 1901, had about one-fifth of the total population of the island. Given the mixed background of the colony, it is not remarkable that municipal problems appear to have dominated the relations between the local population and the colonial authorities in Trinidad at the beginning of the twentieth century. This merely reflects the administrative nature of political action, not necessarily that Trinidad's townsfolk were more politicized or more anticolonial than the rural population. The anticolonial political activity illustrated in Trinidad coincided with similar activity throughout the British Caribbean, in areas equally as well as less urbanized. Urbanism simply does not explain much in the Caribbean at the turn of the century. Indeed, had Magid consulted either of two books by Bridget Brereton not listed in his bibliography—*Race Relations in Colonial Trinidad, 1870–1900* or *A History of Modern Trinidad, 1783–1962*—he would probably have discarded his notion of urban nationalism. At the very least, he would have corrected many of the unfortunate errors included in his text.

FRANKLIN W. KNIGHT
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BRENDA GAYLE PLUMMER. *Haiti and the Great Powers, 1902–1915*. Baton Rouge: Louisiana State University Press. 1988. Pp. xix, 255. \$27.50.

In the early twentieth century as now, Haiti was a poor, economically dependent, and politically unstable small nation in the Caribbean. It was also the living reposi-

tory of its own unique revolution, which created the first independent black republic in 1804. Although seen by the great powers as a problem area inhabited by an inferior race, the very existence of a sovereign Haitian state had enormous symbolic value to the Haitians themselves. Despite their country's vulnerability, the nation's leaders were long successful at playing off the powers against each other. This tactic lost its effectiveness, however, as the United States established unilateral domination of the Caribbean and as Washington grew determined to impose political stability on the region. The Haitians' inability to check a mounting cycle of revolutionary disorder proved fatal; the Americans instituted a military occupation in 1915 and effectively ruled the country until 1934.

The nature of foreign influence in Haiti and the Haitians' ultimate failure to preserve their independence are the focus of Brenda Gayle Plummer's book, but her study differs from conventional diplomatic history. The author casts her net far more widely, attempting to explore "the full range of social, intellectual, and economic exchange among peoples" (p. xii). Her purpose is not only to reach a deeper understanding of events but also to avoid the racially biased, Washington-centered approach that has distanced so many past researchers from Haitian realities and viewpoints. Accordingly, a general introduction to Haitian society is followed by sections on the influential mercantile elite that linked Haitian and metropolitan society, on institutional and ideological development in Haiti, on the arrival and impact of foreign finance capital, on the politics of the preoccupation years, and finally on the onset of the 1915 occupation.

Although the author employs aspects of dependency theory, Immanuel Wallerstein's world-system, Marxist class analysis, sociological theory, and other analytical tools to shape her interpretations, she successfully evades the grip of crude determinism or reductionism. The result is a sophisticated, well-written, sometimes difficult book that is often penetrating and provocative. In addition to its keen observations, the work abounds with a wealth of information about Haitian society and institutions that alone would make it worth reading. The research effort is impressive; Plummer has read widely in diverse Haitian sources and has mined the archives in Washington, Port-au-Prince, London, and Paris. Much of what she found is new to me, and all of it is interesting. Given so many fresh sources, a bibliography would have been particularly valuable, but there is none. The notes, however, are at the bottom of the page, and there are nine pages of illustrations grouped together in mid-text.

The book is innovative, intelligent, and broadly conceived. Strongly researched and incisive, it is a significant contribution to Haitian and Caribbean history and has much to teach students of U.S. foreign relations as well.

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DANIEL PECAUT. *L'Ordre et la violence: Evolution socio-politique de la Colombie entre 1930 et 1953*. (Recherches d'histoire et de Sciences sociales, number 22.) Paris: Ecole des Hautes Etudes en Sciences Sociales. 1987. Pp. 486. 350. fr.

For unknown reasons the history of Colombia has not attracted the interest of many foreign scholars. The number of contemporary foreign scholars who have published works on Colombian history can literally be counted on the fingers of a couple of pairs of hands. And, when they do publish their works, they tend to focus on the period from 1930, when the Liberal party regained control of the presidency, to approximately 1953, when General Gustavo Rojas Pinilla seized control of the government to establish a military regime. David Pécaut's book is the most recent study of that period.

The basic subject of Pécaut's work is precisely what has attracted the interest of foreign scholars to the period, namely, the existence, side by side and intimately intertwined, of democratic order and sociopolitical violence, two seemingly contradictory conditions. Those conditions are unique to Colombia and set its historical development apart from the rest of Latin America. What Pécaut has tried to do is to find an answer to the question of how it was possible for a condition of democratic order characterized by a strong sense of political civility, a traditional two-party system, and respect for constitutional process to coexist with a persistent state of violence that ultimately took the lives of thousands of Colombians. In answering that question, Pécaut, a sociologist, spent twenty years in research. His sources are for the most part secondary works on Colombia's social, economic, and political history. (How could he have missed Richard Sharpeless's *Gaitan of Colombia* [1978] and J. Cordell Robinson's *El Movimiento Gaitanista en Colombia, 1930-1948* [1976]?) His approach is meticulous, analytical, and encompassing, certainly new and intriguing.

Pécaut attempts to demonstrate that it was not just a matter of coincidence that democratic order and sociopolitical violence coexisted in Colombia. Herein lies the most interesting and creative aspect of his book. He theorizes that violence was an essential by-product of a democracy that emphasized "natural" differences, collective allegiances, and networks of social dominance rather than the unity and homogeneity of the society. One of Pécaut's most insightful observations centers on the relationship between such a democracy and the violence that has accompanied it. The problem, he states, cannot be blamed, as so many have done, merely on the so-called oligarchy, that supposedly unholy alliance between the political state and the economic elites for the purpose of exercising control over the masses. He suggests exploring other means of understanding the relationship.

The concluding portion of this work does not point to clear and specific linkages between order and violence, but an unmistakable explanation does emerge. Like other Latin American countries, Colombia em-

barked on a modernization process during the 1930s that should have resulted in the strengthening of the state as the principal agent of the people's welfare. Unfortunately, the modernization process was frustrated by the opposition of private economic interests that gathered into virtual corporate bodies. The failure to strengthen the state, Pécaut argues, led to the entrenchment of a liberal economic process that, although not precluding state intervention in social and economic matters, certainly set limits. Under these circumstances, he maintains that violence became inevitable as the old political passions and rivalries of the nineteenth century were reignited.

Although Pécaut's somewhat complex analysis is hard to follow at times, there is no question that he has made a significant contribution to the study of the period from 1930 to 1953 in Colombian history. The socioeconomic and political relationships that he has proposed as factors in explaining the coexistence of order and violence are not definitive, but they do seem quite plausible and will be hard to refute. I predict this work will stimulate considerable debate among Colombian specialists, who continue to be baffled by the events of those years.

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JEFFREY A. COLE. *The Potosí Mita, 1573-1700: Compulsory Indian Labor in the Andes*. Stanford: Stanford University Press. 1985. Pp. xi, 206. \$35.00.

Jeffrey A. Cole's analysis of forced labor at Potosí from 1573 to 1700 is a welcome addition to recent studies by Enrique Tandeter ("Trabajo forzado y trabajo libre en el Potosí colonial tardío," *Estudios CEDES* [1980]) and Peter Bakewell (*Miners of the Red Mountain: Indian Labor at Potosí, 1545-1650* [1984]). Discovered in what is now Bolivia in 1545, the great colonial silver mines of Potosí quickly attracted a rush of Spanish operators and Indian miners and smelters. By the 1560s, however, the rich surface ores were gone, underground mining had become necessary, and profits had plummeted, causing many of the Indian workers to abandon the site in search of more remunerative employment. At this point, the Spaniards introduced amalgamation, a critical technological innovation that enabled them to process poorer ores than had been possible with smelting. But the dwindling pool of laborers hindered operations.

Drawn primarily from documents in Bolivian, Peruvian, and Argentine archives, Cole's story begins in 1573, when Viceroy Francisco de Toledo established the *mita*, compelling the surrounding provinces to provide Potosí with yearly rotating drafts of forced Indian labor at low wages. Toledo intended the *mita*, Cole argues, as a temporary provision until the Indians returned to Potosí voluntarily, attracted by the renewed profitability of the mines. But amalgamation

required huge capital inputs for mills and mercury, and free laborers spurned the heavy underground work. The *mita* thus remained a central feature of mining at Potosí until the nineteenth century.

Yet, as Cole shows, it satisfied almost no one. Indians resisted, often by migrating beyond the *mita*'s reach. This disrupted village life and oppressed those remaining behind, who were conscripted more frequently than Toledo had ordered or else had to pay for costly substitutes at the free-wage rate. Spanish kings objected to the coercion and abuses of the *mita* but refused to abolish it for fear that royal income from mining taxes might plunge. Local and viceregal officials either avoided responsibility for the *mita* or fought among themselves and with the mine operators over proposed reforms in it. Despite the substantial subsidy in labor and silver that the *mita* afforded them, mine operators and refiners complained that Indian chieftains did not provide full levies of workers. One reformer who threatened to revoke the *mita* subsidy of the most unproductive operators received a fatal dose of poison.

As Cole intended, his work complements those by Tandeter and Bakewell. Cole's institutional history of the *mita* omits consideration of free labor and the economics of mining at Potosí, subjects covered skillfully by Bakewell, who also provided much more detail on the first three decades following discovery. Tandeter deals with the *mita* in the broader context of mining profitability in the eighteenth century. Taken together, the three studies have much to say about the dynamics of the mining economy and its impact on colonial political behavior and Indian society.

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BROOKE LARSON. *Colonialism and Agrarian Transformation in Bolivia: Cochabamba, 1550–1900*. Princeton: Princeton University Press. 1988. Pp. xv, 375. Cloth \$65.00, paper \$25.00.

Andean colonial history has benefited of late from the appearance of first-class monographs on specific regions during well-defined time periods. Brooke Larson's dissertation and previous publications focused on the latter half of the eighteenth century. This book extrapolates both forward and backward, primarily by making use of secondary works, in order to follow, over several centuries, the socio-political-economic evolution of the Cochabamba region and thereby to identify the linkages between the colonial experience and the development of class divisions.

The colonization of Cochabamba in the sixteenth and seventeenth centuries was unusual (vis-à-vis other parts of Alto Perú). Because the valley floor was ideal for agricultural production, Cochabamba had been tightly controlled by the Inca state, leaving the local population without a strong ethnic tradition to fall back on when the Spanish replaced the Inca. The Spanish

reorganized the Indian population to produce food and labor for the silver mines at Potosí. Agricultural production in Cochabamba expanded thereafter, attracting a significant influx of Indian migrants (*forasteros*). When economic fortunes changed for the worse, as the mining center went into sharp decline after 1680, Cochabamba tried to adapt, but it was unable to do so, suffering a serious tax revolt in 1730. Chastened, but intent on maximizing revenue, the colonial state then endorsed the practice of forced distribution of goods to the Indian populations by magistrates. The efforts of native leaders to win reprieves before the appellate court of La Plata were undercut by royal officials in Lima (who were tied to the merchant interests that supplied the distributed goods), and thus the Indian population had to suffer the full force of an oppressive political economy.

Despite the impact of these corrosive economic and political forces, the Andean ideals of self-sufficiency, reciprocity, and collectivity managed to survive, albeit in a much altered form. They did so within a population divided not along local and migrant or even Indian and Hispanic lines but along class lines. This transformation was the marked product of the transfer of land into private hands and resultant distinctions between landholders and peasants. To protect themselves from the vagaries of the landholding class, the peasantry established horizontal-reciprocal relationships, created micro-vertical economic ties, diversified its economic base (for example, by producing bread), and employed other means, including out-migration.

By the late eighteenth century, the landholding class was itself in trouble. Confronted by very expensive transport costs, it was unable to benefit from a resurrection of Potosí silver production. Ever more heavily indebted to religious institutions, the landholders chose to manipulate prices through planned shortages and other artificial measures. Other, more "enlightened," efforts to alter the economic fortunes of the region suffered from capital shortages, violently fluctuating market conditions, political infighting, and ideological flaws.

These processes are synthesized in chapter 9, "Colonial Legacies and Class Formation." Here Larson brings the discussion up to the present, noting that in the twentieth century "peasant resilience" has meant that the sons of small holders go to work in the mines. This resilience should not, she cautions, be mistaken for effective resistance, for, although native Andeans continue to be protagonists in the evolution of Cochabamba society, they have sacrificed something for every transitory victory, losing ground and contact with Andean traditions at each turn.

One aside: the figures should have been prepared by Princeton University Press with greater attention to detail (a map of highland provinces on page 100, for example, has the Pacific Ocean engulfing regions that are now part of Peru). Despite such minor difficulties as this, Larson's book is a significant addition to the field of Andean history, if it is employed—as she herself

suggests—as one contribution (strongest in the middle chapters, which are drawn from primary sources and Larson's own research) to a larger cause. Moreover, although the Cochabamba region is not representative of the Peruvian viceroyalty as a whole, its history is fundamental to an understanding of the processes and effects of colonization, making this book an important addition to the study of colonial societies world-wide.

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MARIFRAN CARLSON. *¡Feminismo! The Woman's Movement in Argentina from Its Beginnings to Eva Perón*. Foreword by GEORGE I. BLANKSTEN. Chicago: Academy Chicago. 1988. Pp. vii, 224. Cloth \$22.50, paper \$10.95.

Books on women's historical experience in Latin America still are rare, and each work is a welcome addition to that body of scholarship. Marifran Carlson's study on the origin and development of the woman's movement in Argentina presents a much-needed survey of the complex and splintered process that brought women into active participation in public life. Carlson examines the woman's movement within the context of and as a response to Argentina's transition from a relatively isolated traditional society to a society confronting the challenges of mass foreign immigration, economic change, and rapid urbanization.

Carlson traces the Argentine woman's movement back to the Bourbon era of the 1780s, when elite women began to take limited roles in civic affairs. That participation set the stage for their deeper involvement with fund raising and the organization of community services during the bellicose independence period. The formation in 1823 of the Beneficent Society by President Bernardino Rivadavia secularized health and human services for women and children and institutionalized a central role for elite women in state philanthropy. The extensive network of services the women created established a pattern of female civic leadership. Inadvertently, the Beneficent Society helped to set the stage for a woman's movement with a feminist agenda.

European immigration and increased educational opportunities for women at the close of the nineteenth century gave rise to a small group of professional women who became the pioneers of Argentine feminism. Philosophically most came out of the free thought movement, which included liberals, socialists, and anarchists who shared a dedication to "universal rationalism" and anticlericalism. The Argentine Association of Free Thought (AAFT) supported full citizenship for women and offered them a rare public forum for expressing their ideas. Members such as Elvira Rawson de Dellepiane, Cecilia Grierson, and Julieta Lanteri-Renshaw used the organization to develop a feminist agenda that often placed them at odds with male members. The growing restlessness of feminists

in the AAFT forced many to turn to the Socialist party. Yet even there they encountered opposition.

During the period 1910–26, feminists organized such groups as the National Feminist Union and the Argentine Association of University Women, which supported women's suffrage, full civil rights, and protective labor legislation. Alicia Moreau de Justo and Lanteri-Renshaw mounted vocal campaigns that included outdoor rallies, petition drives, and mock elections to bring those issues to the public. For their efforts they were ridiculed in the press and received no support from the masses of women. Despite opposition, feminists built alliances with conservatives and leftists and successfully lobbied for protective labor legislation in 1924 and for a reform of the civil code in 1926. During the 1930s feminists continually raised the issue of women's suffrage without success. Carlson points out that by 1940 the feminist movement had come to a standstill because it had not attracted new members, was perceived as elitist, was too closely identified with socialism, and had not responded to the growing sense of nationalism among working-class women.

During the 1940s the outspokenly antifeminist Juan Perón and Eva Perón repackaged the feminist agenda and claimed it as their own. They pushed through sweeping protective legislation, narrowed the wage gap, brought many women into government, and granted women the vote. They won the allegiance of the masses of women in ways that had escaped the feminist movement. Because of their abhorrence for the Peróns' demagoguery and heavy-handed tactics, the leaders of the woman's movement found themselves in the uncomfortable position of opposing legislation that the movement had supported for years.

The inclusion of rare photographs of the leadership of the Argentine woman's movement add to the appeal of this well-researched book. Excerpts from the author's interviews with Alicia Moreau de Justo offer insight into her motivations and an assessment of a life devoted to creating a democratic Argentina in which women could achieve true equality.

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MARK D. SZUCHMAN. *Order, Family, and Community in Buenos Aires, 1810–1860*. Stanford: Stanford University Press. 1988. Pp. xiii, 307. \$39.50.

In this study of Buenos Aires during the first half of the nineteenth century, Mark D. Szuchman identifies a link between the power structures in family and community and the broader issues of political authority and social control. In advancing this argument, Szuchman wants to distinguish his study from the work of other family historians frequently criticized for ignoring the political context informing familial structure.

Szuchman bases his analysis of the various levels of

hierarchical social ordering in Buenos Aires during this period on census returns of two sections of the population from the years 1810, 1817, and 1855. The two sections of population, comprised of the residents of several wards or *cuarteles*, represent a broad selection of social classes, ranging from merchants, professionals, bureaucrats, tradesmen, teamsters, agriculturalists, and the city's *gente de color* to slaves, freed men, shopkeepers, and farmers.

The information Szuchman gleans from the census data with regard to education, household, and neighborhood does not reveal anything particularly surprising, but it shows that the significant differences existing in such small spatial sectors can serve as a microcosmic paradigm for the larger social structures in the whole city. Unfortunately, the information found in this source is not effectively used but serves more as a footnote to the themes addressed. The relationship between the data and the various topics often remains unarticulated so that the general topics come to be treated in an impressionistic fashion.

This problem is particularly apparent in the chapter on education. Although there is valuable information in the chapter on those who had the privilege of education and on the effect of a *cuartel's* social milieu on the quality of education available, there is a lack of thoroughness and consistency, which obstructs our perception of the kind of education available at certain levels and the distinctions between private and state-sponsored schooling. This flaw is unfortunate because it obscures some of the most interesting data.

The failure to articulate a coherent and pronounced argument, informed by and informing the use of the data gathered, diminishes the persuasive force of analysis in most of the chapters. Readers can only feel bewildered as they move through chapters—"Home, Neighborhood, Power," "Disorder and Social Control," "Growing up in the City," "Youth and Challenges to the Moral Order," "Children, Politics, and Education," "The Political Dimensions of Household Changes"—without finding a common thread to give structure and significance to their conjunction in a single work. The conclusion Szuchman draws from his research is also disappointing.

To conclude his unevenly executed examination of youth, education, social control, and politics, Szuchman suggests that his study has provided a better understanding of *caudillismo* (dictatorship) saying that "the Argentine political context in the absence of constitutional norms and of formal and representative institutions for smooth administration required that a time-honored and widely recognized principle of authority be used in lieu of an institutional apparatus" (p. 231). He propounds here the by now well known argument that the patriarchal structure of the family filled the vacuum of governmental authority so that social ordering on the level of the community took the form of a patriarchy with patrimonial families governing the city. This argument has been advanced more effectively in several other studies. (See, in particular, Susan So-

colow, *The Merchants of Buenos Aires, 1778–1810* [1978]; and Robert B. Oppenheimer's essay in *Population Growth and Urbanization in Latin America: The Rural-Urban Interface*, ed. John Hunter et al. [1983]). Although Szuchman's study succeeds in elucidating the acceptance of the Rosas family's regime by sections of the population, it overemphasizes the character of their rule as being significantly different from that of other regimes.

Despite these flaws in the study, the data are valuable. The chapter on social disorder provides interesting glimpses of the society during this time, and it also makes good use of police records. I emphasize the importance of the data from the *cuarteles* because they provide valuable views on the differentiation of social structure in so circumscribed a space, which underscores the importance of both spatial studies and the *cuartel* as a unit of social and historical study.

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JAMES R. SCOBIE. *Secondary Cities of Argentina: The Social History of Corrientes, Salta, and Mendoza, 1850–1910*. Edited by SAMUEL L. BAILY. Foreword by INGRID WINTHER SCOBIE. Stanford: Stanford University Press. 1988. Pp. xvi, 276. \$42.50.

This book is the last and, in many ways, the most ambitious study undertaken by a fine historian, James R. Scobie. It offers the first comparative work dealing with Latin American cities. The volume was researched in 1975–76 and partially written at the time of Scobie's death in 1981. Samuel L. Baily has accomplished a rather remarkable feat by completing the last half of the book and making it almost impossible to discern that more than one author has been involved.

Using locational theories that underpinned his earlier study of Buenos Aires, Scobie added dependency to explain why secondary cities in Argentina, specifically Corrientes, Salta, and, to a lesser extent, Mendoza did not experience sustained economic growth and diversification from the railroad and streetcars, development of the agricultural hinterland, and population changes wrought by immigration. Of the three cities, only Mendoza offered prospects of economic development and social mobility by 1910, and those hopes resulted mostly from the expansion of grape cultivation and wine production.

Scobie attributed the sharp contrast of these cities with Buenos Aires to their strong economic dependence on the national capital for goods and services. Locational factors, such as the impact of the horrendous 1861 earthquake in Mendoza, Corrientes's unsuitable position as a port and commercial center, and Salta's isolation from national markets because the railroad did not reach the city until 1891, reinforced the consequences of dependence. Scobie also offered a cultural explanation by suggesting that native-born Argentines lacked value systems and material resources necessary to spur development. Mendoza, the most

attractive of the three cities, had the largest number of European immigrants, while Corrientes, the most stagnant city, had increased numbers of Paraguayan immigrants. As Scobie put it, "It was not the presence of foreigners per se in the population that contributed to growth, but the presence of foreigners who, like many South Europeans, could bring in ideas, technology, and capital" (p. 134).

Through manuscript censuses of 1869 and 1895, augmented by extensive documentation in local newspapers, archives, and official publications, Scobie reconstructed the physical, social, and economic history of these three Argentine cities. The result is impressive because it includes observations that emphasize the breadth and depth of knowledge that he brought to the study of urbanization. One is struck by the role that public health played in these cities. If there was little economic development to spur physical expansion, fears of illness offered great incentive for change, particularly among elite families. Thus, the consequences of contaminated water tables and the need to escape endemic diseases present in low-lying areas profoundly influenced residential patterns, even more than the social relationship to plazas that traditionally affected locational decisions. The absence of a significant middle class also had its impact by limiting opportunities for social mobility. Rigid class cleavages in secondary cities had impeded urban growth by 1910.

Despite the sophistication and expertise evident in this work, a full knowledge of these cities awaits additional studies of social patterns that cannot be ascertained from census data and newspapers, which offer information about the poor that is not as complete as information about the rich. Elite fears about the potential criminality of the working class are described without any discussion of the reality of crime. Public health is identified as a major concern, but municipal politics beyond water and sewer policies were not analyzed except for the regulation of prostitution in Mendoza. Human costs, particularly to the lower class, and the financial burdens that public health demanded cannot be ascertained. In fact, no municipal or provincial budgets were cited, nor were national debates about protective tariffs for provincial industries or the problems of public health throughout the republic. Generally speaking, the role of politics and the role of the state are ignored, yet their relationship to urban development cannot be denied. It is difficult for the reader to surmise whether Scobie would have contemplated these issues had he lived to complete the manuscript. Yet, even with these shortcomings, this book is a valuable contribution to urban history and Argentine studies.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

RONALD HAYCOCK and KEITH NEILSON, editors. *Men, Machines, and War*. Waterloo, Ont.: Wilfrid Laurier University Press. 1988. Pp. xx, 219. \$35.00.

RONALD HAYCOCK and KEITH NEILSON, Preface. WILLIAM H. MCNEILL, Men, Machines, and War. HOWARD BAILES, Technology and Tactics in the British Army, 1866–1900. DOMINICK GRAHAM, Observations on the Dialectics of British Tactics, 1904–45. JON SUMIDA, The Royal Navy and Technological Change, 1815–1945. CHARLES MESSENGER, The Influence of Technology on Airpower, 1919–45. DENNIS E. SHOWALTER, Prussia, Technology, and War: Artillery from 1815 to 1914. G. R. LINDSEY, Technology, Society, and International Security since 1945. R. G. HAYCOCK, Creating Volcanoes Everywhere: Australia's Owen Gun Story.

LLOYD J. MATTHEWS and DALE E. BROWN, editors. *The Challenge of Military Leadership*. Foreword by WALTER F. ULMER, JR. (AUSA Books.) Washington, D.C.: Pergamon-Brassey's, for the U.S. Army War College Foundation. 1989. Pp. xviii, 167. Cloth \$32.00, paper \$14.95.

WALTER F. ULMER, JR., Introduction. OMAR N. BRADLEY, On Leadership. DONN A. STARRY, Running Things. JAY LUYAAS, Napoleon on the Art of Command. JOHN T. NELSEN II, *Auftragstaktik*: A Case for Decentralized Combat Leadership. ARTHUR S. COLLINS, JR., Tactical Command. LARRY H. INGRAHAM, Fear and Loathing in the Barracks—And the Heart of Leadership. JOHN M. VERMILLION, The Pillars of Generalship. CHARLES G. SUTTEN, JR., Command and Control at the Operational Level. MITCHELL M. ZAIS, Strategic Vision and Strength of Will: Imperatives for Theater Command. JOHN F. SHORTAL, MacArthur's Fireman: Robert L. Eichelberger. THOMAS R. STONE, General William Hood Simpson: Combat Commander. PHILIP LEWIS *et al.*, Defining Character in Military Leaders. LEWIS SORLEY, Doing What's Right: Shaping the Army's Professional Environment. CLAY T. BUCKINGHAM, Ethics and the Senior Officer. DONALD F. BLETZ, The "Modern Major General."

R. J. W. EVANS and HARTMUT POGGE VON STRANDMANN, editors. *The Coming of the First World War*. New York: Clarendon Press of Oxford University Press. 1988. Pp. viii, 189. \$45.00.

MICHAEL HOWARD, Europe on the Eve of the First World War. Z. A. B. ZEMAN, The Balkans and the Coming of War. R. J. W.

EVANS, The Habsburg Monarchy and the Coming of War. D. W. SPRING, Russia and the Coming of War. HARTMUT POGGE VON STRANDMANN, Germany and the Coming of War. RICHARD COBB, France and the Coming of War. MICHAEL BROCK, Britain Enters the War.

RICHARD WALL and JAY WINTER, editors. *The Upheaval of War: Family, Work, and Welfare in Europe, 1914–1918*. New York: Cambridge University Press. 1988. Pp. vii, 497. \$65.00.

RICHARD WALL and JAY WINTER, Introduction. JAY WINTER, Some Paradoxes of the First World War. RICHARD WALL, English and German Families and the First World War, 1914–18. REINHARD SIEDER, Behind the Lines: Working-Class Family Life in Wartime Vienna. PETER SCHOLLIERS and FRANK DAELEMANS, Standards of Living and Standards of Health in Wartime Belgium. ARMIN TRIEBEL, Variations in Patterns of Consumption in Germany in the Period of the First World War. PETER DEWEY, Nutrition and Living Standards in Wartime Britain. ALASTAIR REID, The Impact of the First World War on British Workers. PATRICK FRIDENSON, The Impact of the First World War on French Workers. JEAN-LOUIS ROBERT, Women and Work in France during the First World War. UTE DANIEL, Women's Work in Industry and Family: Germany, 1914–18. DEBORAH THOM, Women and Work in Wartime Britain. MARIE-MONIQUE HUSS, Pronatalism and the Popular Ideology of the Child in Wartime France: The Evidence of the Picture Postcard. RICHARD SOLOWAY, Eugenics and Pronatalism in Wartime Britain. CORNELIE USBORNE, "Pregnancy is the Woman's Active Service": Pronatalism in Germany during the First World War. PAUL WEINDLING, The Medical Profession, Social Hygiene, and the Birth Rate in Germany, 1914–18. JÜRGEN REULECKE, Männerbund versus the Family: Middle-Class Youth Movements and the Family in Germany in the Period of the First World War.

ARTHUR MARWICK, editor. *Total War and Social Change*. New York: St. Martin's. 1988. Pp. xxi, 134. \$39.95.

ARTHUR MARWICK, Introduction. JAMES F. MCMILLAN, World War I and Women in France. ALASTAIR REID, World War I and the Working Class in Britain. WOLFGANG J. MOMMSEN, The Social Consequences of World War I: The Case of Germany. PAUL DUKES, The Social Consequences of World War II for the USSR. MARK ROSEMAN, World War II and Social Change in Germany. FRANÇOIS BEDARIDA, World War II and Social Change in France. PENNY SUMMERFIELD, Women, War, and Social Change: Women in Britain in World War II. ARTHUR MARWICK, Conclusion.

COLIN MCINNES and G. D. SHEFFIELD, editors. *Warfare in the Twentieth Century: Theory and Practice*. Boston: Unwin Hyman. 1988. Pp. xiv, 239. Cloth \$49.95, paper \$16.95.

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REVIEWS OF BOOKS

TO THE EDITOR:

Paul Faler's review of my book, *The Paddy Camps: The Irish of Lowell, 1821-61* (AHR, 94 [October 1989]: 1172-73), is both curiously narrow and surprisingly idiosyncratic.

Faler begins his analysis with a lengthy critical examination of a number of the editorial decisions reached and the research approaches taken during the development of the book. In one example, he faults a map, the "Plan of the Town of Lowell and Belvedere Village, 1832," as "mostly unreadable, leaving the reader unable to use the street diagrams to locate the Irish neighborhoods within the city." That was hardly the purpose of the map, which was placed within the book only to offer the reader an overview of early Lowell and was to be used in conjunction with its counterpart on the following page. This second map showed the location of the Irish neighborhoods clearly, oriented in a fashion similar to the first and with specific reference to Lowell's power canals. In a second instance, Faler confusingly asserts that I ignored his self-pronounced "rules of good scholarship" by citing population levels from published secondary sources, although he later compliments the piece for "excellent information on changing Irish occupations and family employment patterns . . . carefully compil[ed] from census and mill records." In a third example, he decries the use of "controversial and self-serving quotations . . . presented as fact without corroboration" but, sadly, never specifies where these citations were in the text. Finally, in the most shocking assertion of all, Faler speculates that "the research is inadequate." Where? What records exist that might have been used? Once again, Faler makes a sweeping charge without documenting what he means.

Setting aside these questions, Faler next discusses his view of the focus of the book. Let me respond by indicating first that I have a great respect for Faler's research on the shoe industry. Like others, I have looked forward eagerly to his next study, especially if this new publication should incorporate his reflections

on nativism, but it is profoundly disappointing to see my work on the development of an ethnic community judged by the standards of his as-yet-unpublished research or on the basis of research shared only with his students. It is also unfair for Faler to concentrate so heavily on the period of roughly 1850-1855, ignoring both the general period under consideration and, indeed, the focus of the book itself. He has a responsibility to assess *The Paddy Camps* on the basis of my research and not largely on where, for a relatively brief moment, our mutual research and teaching interests intersect.

Let me correct a few of the more telling inaccuracies. First, *The Paddy Camps* is not a "single-generation study" but one that spans forty formative years in the development of an ethnic community in which Irish children play a crucial role. In addition, it was never my intent to produce "a rich social history of the Irish immigrants and their adversaries" but only of the Irish—pre-Famine, Famine, and American-born—and often in terms of relationships that, especially before the late 1840s, were not typically adversarial.

As to Faler's mistaken assertion that the relationship between the Irish and the Yankee upper class is unexplained, I am frankly puzzled. I assume that Faler is referring to Lowell's Yankee middle class, since money made in Lowell typically went elsewhere. As I pointed out, the role played by absentee Boston capitalists in changing the pattern of employment contributed greatly to rising nativism in Lowell around 1850. Beyond that, I discussed the relationship between Lowell's Yankees and the Irish in terms of the feature that determined their relationship—employment—and also by demonstrating social relations through such pivotal events in their mutual history as the annual dinners of the Lowell Irish Benevolent Society, the dedication and rededication of St. Patrick's church in 1831 and 1854, and, especially, the famous 1835 school compromise.

Finally, let us examine the snippet of the history of the Irish community that Faler does discuss in detail. My intent was to address the impact of nativism on the Irish from the Irish perspective, whenever possible. This was done, although I took great care to research sources that represented all sides. Using voting tallies in the *Lowell Advertiser*, I noted that the Irish continued to split their vote (roughly 300 Whig, 300 Democrat) through 1854 and that the Irish did not ally in lock-step fashion with the Whigs. Rather, politics in this period of turmoil was far more localized and personal. In the face of rising nativism, a contingent of Irish priests—the so-called O'Brien Dynasty—emerged as the new political leaders of the Irish community, although none

ever held elected office. The effect was to produce the next stage of development in Lowell's Irish community, blending all Irishmen into an Irish-American and Catholic subculture. Strangely, Faler never mentions the crucial role of the Catholic church in his review.

Despite its shortcomings, Faler's review serves nobler purposes. It demonstrates that historians interested in questions of class and those who write about issues of ethnicity must look to the commonalities to produce a rich social history of immigrant working-class communities. Faler's review also speaks to the need for reviewers to respond to a study in the manner in which the author defines that study and not in relation to one's own research and teaching interests. Finally, Faler's idiosyncratic approach addresses the need to establish what I call "standards of good reviewership." Although this is the first critical response to a book of mine that has been reviewed widely, I believe that the principle of a critical review is an honorable and necessary one. But there must also be standards that define this criticism, that push our common knowledge of ethnic communities along paths of more informed scholarship. Among these standards, historians have a responsibility to produce an assessment that is fair, accurate, informed, reflective, balanced, well-documented, courteous, and—above all else—comprehensive.

BRIAN C. MITCHELL
Arlington, Virginia

PAUL FALER REPLIES:

In my original review of *The Paddy Camps*, I made a number of criticisms that Brian Mitchell regards as narrow, idiosyncratic, or unfair. Overall, I found the book disappointing, despite portions that are interesting and valuable. In the following few paragraphs, perhaps I can expand on these observations.

I did not find the book well written. The author relied too heavily on secondary sources. There are places in which footnotes do not correspond to the text. And, as one who has worked in the area of nineteenth-century Massachusetts history, I found it frustrating that certain important or controversial statements had few citations of evidence. A few examples:

If "the faction" was "fundamental to Irish social structure" (p. 23), what is a faction? How many factions were there in Lowell and who belonged to them? If the faction was "fundamental," why does Mitchell introduce the concept and then abandon it for mostly a class analysis?

Where is the evidence that financial backing for the first Catholic church in Lowell came primarily from the small middle-class element (p. 41)?

He sees close relations between Bishop Fenwick and Kirk Boott, agent of the Boston Associates and virtual ruler of early Lowell. Mitchell reports that they met privately "on numerous occasions" (p. 53) but cites no evidence for the assertion.

What happened to the Irish middle class that was so influential in early Lowell? It seems ironic that, as the

Irish middle class became larger, it became weaker and more subservient to domineering priests whom it had once successfully resisted. And, in charting the growth of that middle class, why not use the Lowell tax records?

Reverend John O'Brien was the key figure in Lowell after 1848. He molded what Mitchell calls "an Irish Catholic identity" but which Mitchell leaves undefined. O'Brien "abandoned the assimilation philosophy of the Irish middle class, arguing instead for a more pluralistic perspective" (p. 126). No evidence for O'Brien's argument appears: no letters, sermons, or testimony to show the case O'Brien "argued." Yet he was an energetic figure at the center of many controversies.

The section on politics is weak, less than a page in the entire book. Yet, for perhaps no other group in American history has electoral politics been more important—or rewarding—than for the Irish. Although their ascent to power came after 1860, the Irish political apprenticeship began in the period of Mitchell's study, 1820–1860. He leaves the topic unexplored and unexplained. Ben Butler of Lowell, a wizard of Massachusetts politics and certainly no enemy to the Irish, blamed the Irish alliance with the conservative Whigs for the defeat of the constitution of 1853 and the emergence of nativism in 1854. Mitchell should address that issue. Was there an Irish alliance that fueled working-class anger and resentment?

The powerful nativist upsurge of the 1850s is another critical issue that Mitchell describes with clichés and folklore rather than careful and objective analysis. Historians should avoid terms or names that are highly partisan and pejorative. In 1855, the Massachusetts legislature created a "Joint Special Committee on the Inspection of Nunneries and Convents." Mitchell calls the body the "Smelling Committee" (p. 139), without ever telling the reader either the official name of the committee or the source of the pejorative term he adopted.

Going directly to the census for population figures is not my self-pronounced rule of good scholarship. To use the original census rather than secondary works is a sound procedure. Why cite Stephan Thernstrom's study of Newburyport for the number of Irish in Lowell? And, when Mitchell does use the census for his data on Irish employment in 1860 (Tables 13–15, pp. 13–15), the tables do not show the Irish as the majority of the mills' 13,000 workers.

The street diagram (p. 44) he includes to show the Irish neighborhoods of early Lowell is of little use because the streets mentioned in the text do not appear on the diagram. He mentions Lowell Street six times (p. 45), but the diagram does not show Lowell Street.

If these criticisms are narrow and idiosyncratic, Mitchell has just cause for complaint. But the standards I applied were ones I thought common to good scholarship.

PAUL FALER
University of Massachusetts,
Boston

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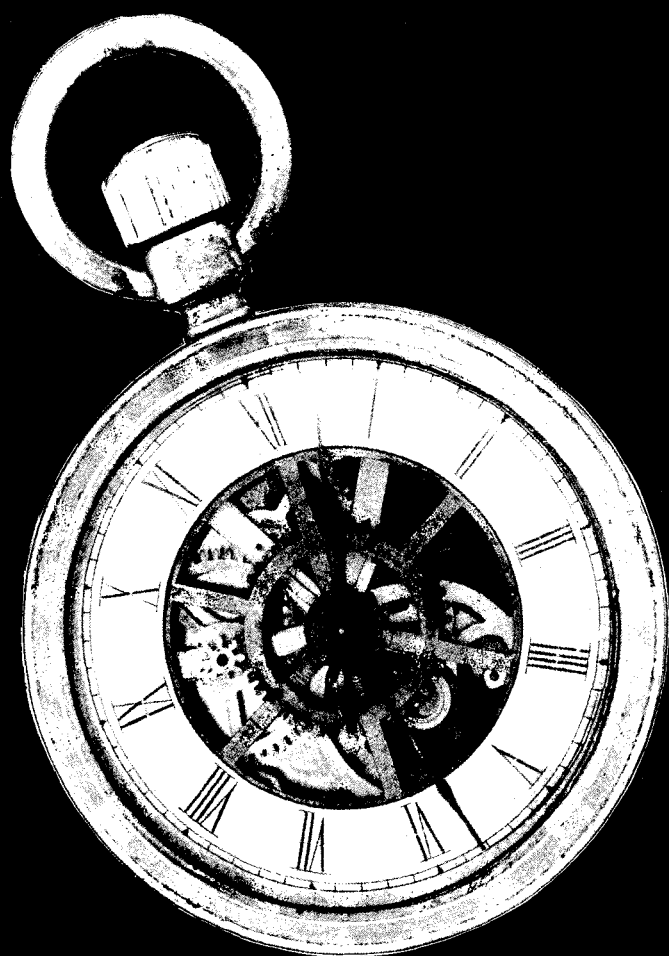
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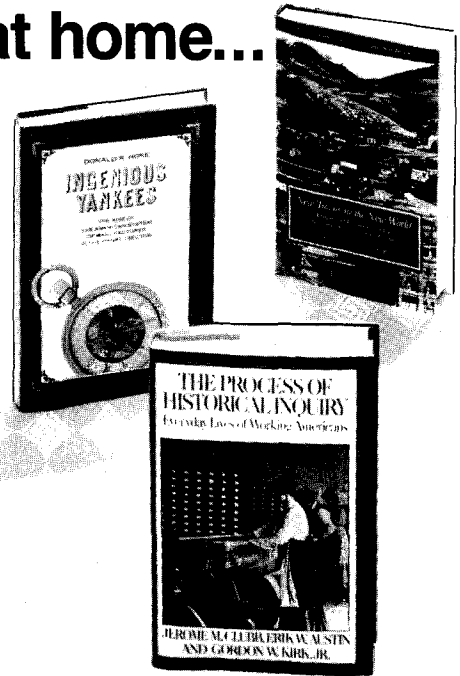
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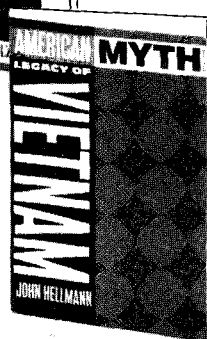
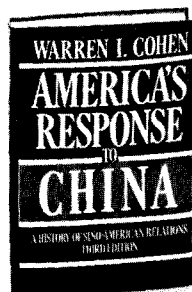
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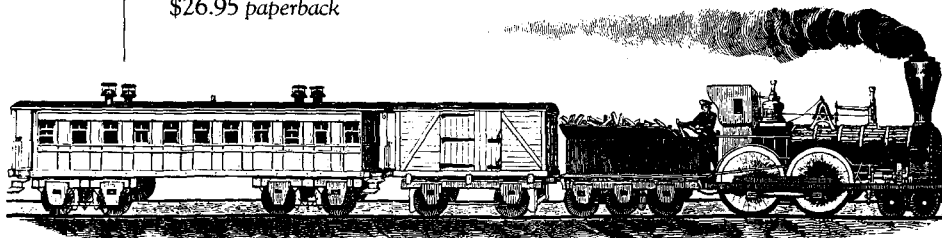
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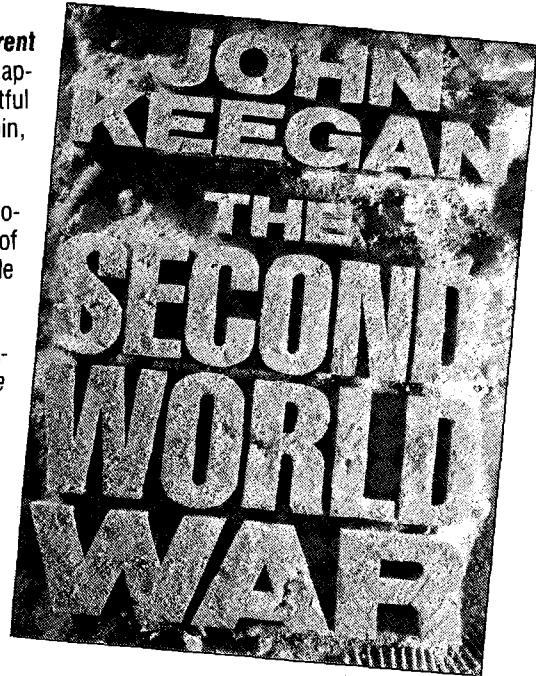
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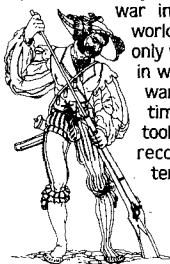


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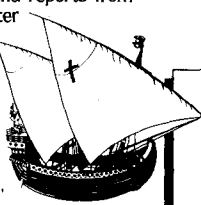
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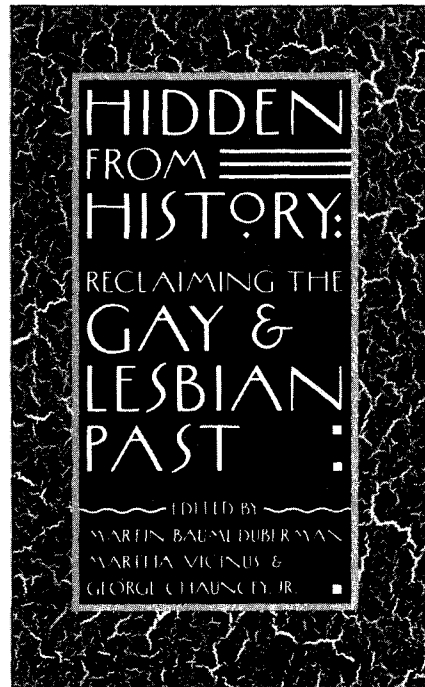
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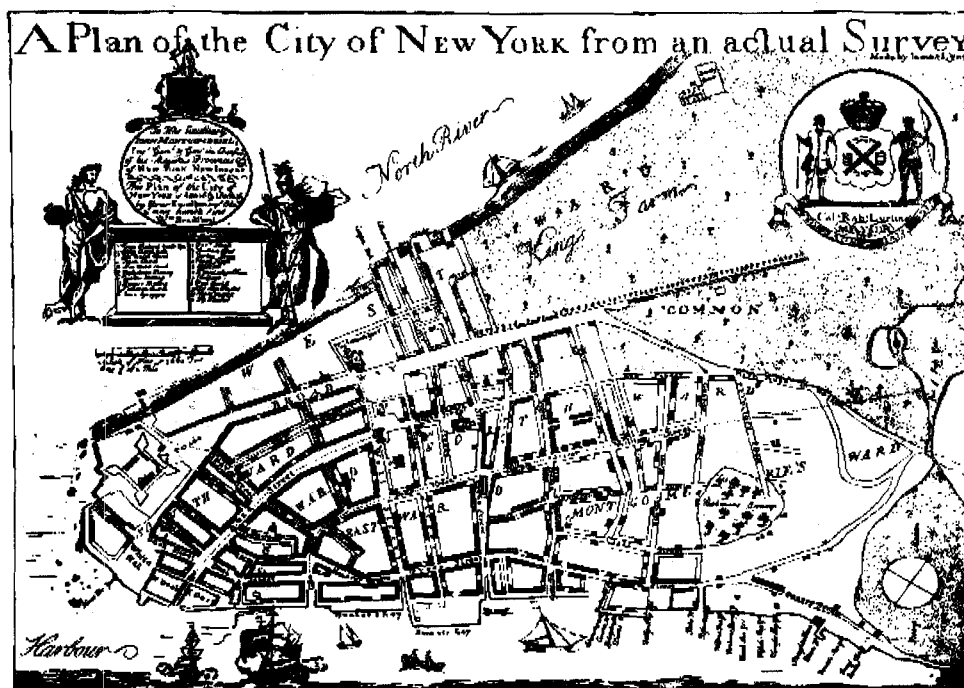
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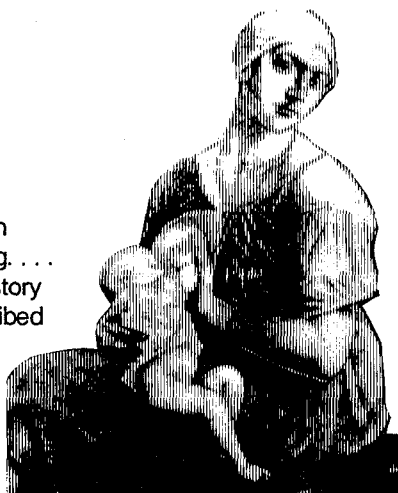
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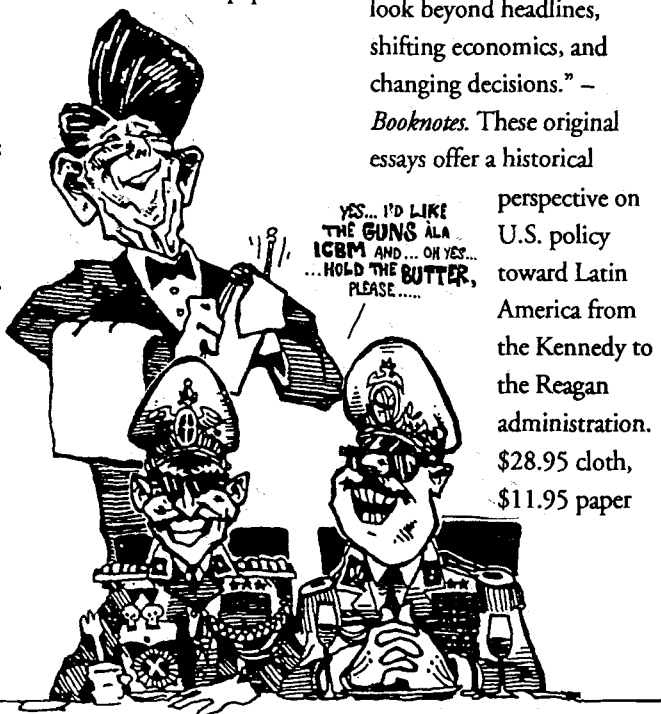
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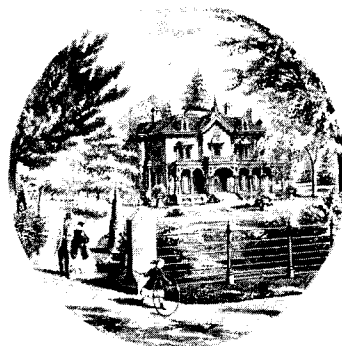
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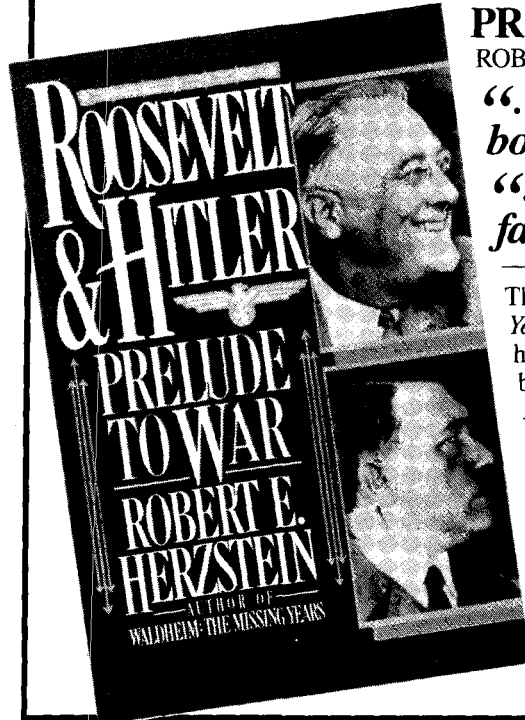
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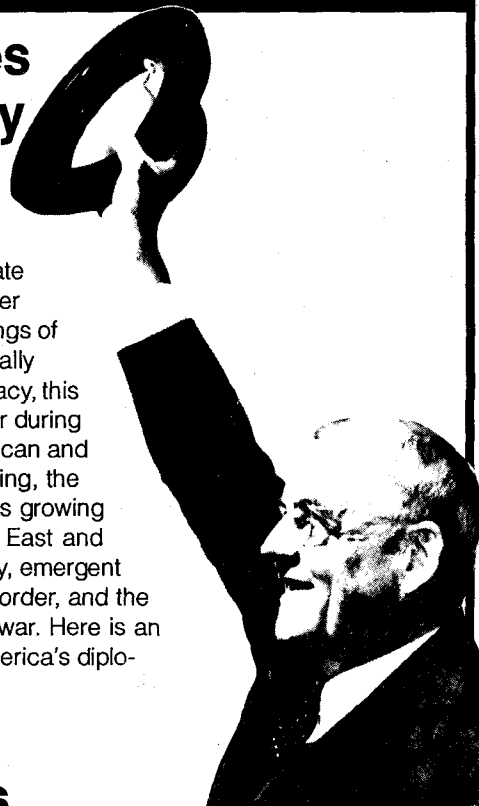
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